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WHERE DOES PARAPSYCHOLOGY GO NEXT?

BY ROBERT H. THOULESS

[Address delivered to the meeting of the Parapsychological Association in New York on 10 September, 1959]

I AM proposing to restrict myself to one aspect of the topic suggested by the title of this address and to ask what is the situation of parapsychological research in the light of criticisms that are currently made of it. Do these criticisms themselves suggest any new paths for exploring?

Many of the critics themselves would say that where we should go next is obvious. They would consider that we should pack up our bags and go home and admit that our hunting expedition has been merely a chase of shadows. Either we have been misled into seeing causes behind mere chance coincidences or we have accepted evidence for what a sensible person would have known beforehand was an impossibility.

The best-known critic of the first type is Mr Spencer Brown who, first in an article in *Nature* (1) and later in the appendices of his book *Probability and Scientific Inference* (2) suggested that results in parapsychological experimentation are merely examples of the oddities that occur in any attempt to randomize. He suggests, therefore, that these are not correctly interpreted as evidence of some unknown effort of communication, but only of the failure of current probability theory to give a correct measure of the degree to which such oddities are to be expected.

The other critics of parapsychology do not suppose that its results can be so easily explained. They realize that the results of the most striking parapsychological experiments indicate some cause at work. They consider however that an explanation in terms of ESP and other *psi* processes are explanations in terms of

impossibilities. They feel that an improbable explanation is to be preferred to an impossible one, so they explain the results as due to fraud on the part of someone.

This line of criticism was started by Dr George R. Price of the Department of Medicine at Minnesota in an article published in 1955 (3). It has more recently received strong support from Dr C. A. M. Hansel of the Department of Psychology at Manchester University, who wrote an article in *The New Scientist* in 1959 (4). There are differences between the approach of Price and that of Hansel; but they both start from the assumption that the parapsychological type of explanation is impossible, and they both (somewhat inconsistently) demand one crucial experiment in which additional precautions shall be taken. I say 'somewhat inconsistently' because it would seem that if a thing is absolutely impossible, no experimental design would make it reasonable to accept it. Hansel amusingly suggests an analogy between the acceptance of ESP and a belief that the lady has really been sawn in half by a conjurer. If this analogy is strictly interpreted it should be obvious that no improvement in the conjurer's experimental design would convince us that a lady who afterwards appeared on the stage had really been sawn in half.

The logic of the argument of these writers is unassailable if one admits their two premises—that what is impossible does not happen and that ESP is impossible. The first of these is a tautology; no one will doubt its truth and one may be surprised to learn that Dr Price only became convinced of it by reading Hume's argument on miracles. The doubtful premise is the second one. How can the impossibility of ESP be known? Which is the more reliable way of finding out: to trust to an intuition of its impossibility or to do experiments to find out whether ESP events do in fact happen?

The fact that such criticisms as these are themselves open to criticism does not rob them of all importance. It may still be the case that they are genuine expressions of a widespread and even not altogether unfounded uneasiness about the position of parapsychological research. The parapsychologist should not allow himself to suppose that, in order to refute the arguments of his opponents, it is sufficient to demonstrate the critic's prejudice and his frequent ignorance of the important landmarks of parapsychological research. The criticism may nevertheless be a sign of something deeper: an unwillingness to accept the surface explanation of parapsychological experiments as indicators of a non-sensory mode of communication. This is felt to be difficult by many in the field of scientific research whose judgment one

must respect. It is a real difficulty which cannot be got over merely by pointing out crudities, inconsistencies and ignorance in the published criticisms. Rather, I would suggest that every criticism, even if it appears unreasonable, can be taken to point to new directions of research.

I do not mean that I think research ought to be directed towards directly meeting the objections of our critics. I agree with Professor Rhine that parapsychologists would be wasting their time if they accepted Dr Price's invitation to conduct an experiment with ESP cards in welded steel containers and the result judged by a jury of strongly unbelieving scientists (5). Such an experiment would be a waste of time not only because we could have little confidence in it being fruitful of any positive results; but it is also open to the objection that if it did have a positive result it would carry us no further. If the evidence of Dr Soal and other successful parapsychological experimenters is rejected because what they claim to demonstrate is thought to be impossible, it would be necessary also to reject the testimony of twelve strongly unbelieving scientists. Price's reply that in that case the jury must be increased in number is mere trifling. No such experiments could carry conviction to strong unbelievers, but other ways are open. Many years ago Dr Lucien Warner showed a better way for the sceptical scientist to become convinced as to the reality or otherwise of ESP (6).

Dr Price was no doubt right in saying that one can never be completely certain that any individual or group of individuals has not deceived one. I cannot be certain that Dr Soal and his witnesses did not conspire to commit a fraud. If Dr Price collected his twelve hostile critics and together they carried out a successful ESP experiment, I still could not be certain that they had not conspired to defraud me, their hostility being all pretence. What, however, I can be certain of is whether, if I have carried out a successful ESP experiment, I have or have not committed fraud or entered into a conspiracy to defraud. Similarly Dr Soal knows with certainty whether or not he entered into conspiracy to defraud, and so would Dr Price know of an experiment he had carried out himself.

The way that Dr Warner dealt with this situation was by doing a successful experiment himself. He employed a percipient who had already shown her ability to succeed in ESP tests. He took adequate precautions against the percipient cheating him and against unconscious deception on his own part. He had the percipient in a locked room on a different floor from the experimenter, with communication only by signal from percipient to

experimenter and not the other way. In 250 guesses by GESp he got 93 hits, nearly twice mean chance expectation, with the respectable probability against chance occurrence of less than one in a thousand million.

This, I suggest, is one thing that parapsychologists might well do with high-scoring subjects: not waste them by intensive experimentation until their scoring has dropped to chance level. It would be better if we should conserve their gifts so they can be used by sceptics who are willing to be convinced in the Lucien Warner way. We should be in a stronger position against criticism of Dr Price's and Dr Hansel's type if we could say: 'Well, here is X. He is at your disposal for an evening's experiment. Introduce any precautions that seem to you to be necessary, only be reasonable and don't antagonize him by showing any hostility. Explain your precautions to him and why you take them; don't treat him as a criminal under suspicion. Then we can't be certain but it is most likely that he will give you positive results.'

This would be a better and an easier way than Price's suggestion of sealed boxes and hostile jury. It is up to parapsychologists to try to provide the means of carrying it out.

Since high-scoring subjects are much less common than we should like, another future line of research that this type of criticism suggests is trying to get reliable control over ESP and other paranormal occurrences. It would be much easier to demonstrate repeatability of *psi* if we could make our own high-scoring subjects at will.

There are many ways in which this may be attempted. One way is by the use of drugs. It is of no special interest, except perhaps as a pilot experiment, to find out that the use of a certain drug gives a just significantly better score than the subjects could get without it. What we want is a drug which we can give to our subjects with a reasonable certainty that they will score something like twenty per cent over mean chance expectation after having taken the drug. No such drug has yet been discovered and there seems to be at present no indications that any drug looks as if its use will produce this result. Nor has this result so far been obtained by any method of hypnosis.

The method I am inclined to follow myself is that of self-experimentation. I feel that if I could find out for myself what condition produced high ESP scoring in me, then it would be possible to induce that same condition in other people. Certainly it is not easy. When I was engaged in the same work on psychokinesis I put forward the formula that probably the right attitude

of mind for successful scoring was that which would be expressed verbally as 'I want to succeed, but I don't really care a damn whether I do or not.' I am still inclined to think that some such attitude of mind is what is required. I am encouraged to find that in Professor Herrigel's book on Zen Buddhism and Archery he suggests that the condition for success in archery is much what I have described for *psi*, although he expresses it in a more elegant verbal formula. He speaks of 'selfless purposelessness' and says that the right way of shooting an arrow is 'by letting go of yourself, leaving yourself and everything yours behind you so decisively that nothing more is left of you but a purposeless tension.'

I have given myself the task of guessing five different ESP cards in a sealed package. The chance here of succeeding by chance is, of course, 1 in 120. I have tried to attain Herrigel's state of selfless purposelessness in guessing and have also done a control series of guesses in which I have used the more ordinary Western method of trying to succeed as hard as I could. So far I have not succeeded by either method. I am not wholly satisfied with the task, which seems to me to be a somewhat unstructured one.

The record of my failure to train myself to score reliably in PK is contained in an article on psychokinesis with dice which I published in the *Proceedings of the Society for Psychical Research* (8). I may be publishing in a few years time the record of my failure to train myself to succeed in ESP tasks. Yet even if I fail I think that the pursuit of this aim is worth while. Others may be more successful than I have been.

In considering the possibility of training oneself in ESP tasks one ought not, I think, to neglect the spontaneous evidence we have that these tasks may be associated with a level of spiritual development which most of us have not reached. Paranormal events do seem to follow saints and holy men in all religions. It may be that we should succeed more easily if we reached their stage of spiritual development. It may be, of course, also true that if we did so, we should no longer be interested in demonstrating the reality of *psi*. When I speak of spiritual development, I do not mean such trivialities as the ability to go into a state of trance by contemplating one's own navel. I mean rather the development of detachment, selflessness, renunciation of the fruit of works, and selfless love of one's fellows. It may be that the hostilities, the ambitions, and the self-seeking which we inherit from our culture may be in a special way antagonistic to the development of the ability to succeed in *psi*. Success as psychological experimenters may depend not only on experimental techniques but also on personality development in this dimension.

Another type of criticism is that which suggests that the supposed positive results of parapsychology are mere statistical artifacts. Publicity to this idea has been given in recent years by Mr Spencer Brown's work (1, 2). So far as it bears on parapsychology, the work of Brown may be divided into two parts: (1) its claim to prove that the theoretical basis of ordinary probability theory is unsound, (2) its claim to show that the expectations of ordinary probability theory are in fact contradicted by observation of how frequently one finds in random matchings the kind of coincidences that have been supposed to be evidence for ESP or PK.

I do not propose to say much about the first of these lines of attack. Mr Brown seems to think that the targets in parapsychology have to be in a special kind of order, a 'random' order which is free from pattern. What is, in fact, required is that they shall be subject to a randomizing process which is equally likely to produce any order, whether patterned or not. If this is realized, Mr Brown's theoretical discussion is seen to have no relevance to parapsychological experimentation.

His claim to have shown empirically that the expectations of ordinary probability theory are not fulfilled in practice seems, at first sight, to have more force. But we must approach evidence against orthodox probability theory with the same caution as we should approach a new finding in ESP. First we must ask how far the evidence is selected. Spencer Brown records matchings that show a low chance expectation as ordinarily calculated. He does not, however, record how many matchings he made that showed no abnormality from the point of view of ordinary probability theory. We must not forget that ordinary probability theory indicates such small values of P will be found occasionally in random matchings and the more matchings we make the more likely we are to find them; so the occasional occurrence of small P 's is not necessarily evidence against ordinary probability theory.

One would, for example, be surprised to find an abnormality with a P value of $\cdot 001$ in a single observation and we should rightly consider this as good reason for suspecting some cause at work. But it would be more likely than not that such a P value would arise at least once in a thousand observations and the probability of a P value of this sort occurring even in a hundred observations is too high to be regarded as significant of a cause.

The most striking abnormality in chance matchings pointed out by Brown was one found in Oram's work on matching in random number tables in which a quartile deviation was observed whose probability of chance occurrence was 1 in 7,340 (9). This sounds

startling if presented as an isolated observation, but several questions must be answered before we can regard our surprise as justified. (1) Amongst how many observations was this found to be the most striking? (2) Even if this were found amongst so small a number of observations that we should be justified in being surprised, which is the more likely, that the ordinary probability theory is wrong or that a long odds has by chance in this case come off? Those who are not convinced by Mr Spencer Brown's theoretical arguments may well think that the latter is more likely. But we really have no data given us for deciding the first question. We know that in Oram's own work fourteen comparisons were made of which this was the most striking. If Mr Spencer Brown has made a thousand matchings of which this is the most striking, ordinary probability theory would not lead one to regard it as very unexpected. By ordinary probability theory, if he goes on long enough he should find by mere chance odds much longer than this. But the odds observed here are incomparably smaller than those found in the best ESP experiments. It would be billions of years that Mr Spencer Brown would have to go on before one would expect by chance that he would find an abnormality of which the odds against it occurring by chance were of the same order as, let us say, the Soal-Goldney ESP experiments on Basil Shackleton (10).

Mr Brown seems indeed to know this, since in a discussion at the Ciba conference he admitted that the Soal-Goldney results are not capable of being explained in accordance with his theory. His exact words were: 'If more of us could get results like Dr Soal, there would be no difficulty at all in accepting a communication hypothesis (11). But there can be no sound reason for restricting this exception to the Soal-Goldney work. What is true of that is true also of the Pearce-Pratt experiments, the Lucian Warner experiment already mentioned, and very many others. This exception in favour of Dr Soal's work does not appear anywhere to be mentioned in Mr Brown's book.

It may be said that most of the published experimental ESP work shows levels of chance expectation much worse than the 1 in 7,340 of Oram's matching experiment. This is true, but most of the published experimental work does not, and is not intended to, provide evidence for the reality of ESP. It may have, however, other quite legitimate purposes. It may be an indication of a highly probable result which it is hoped will be confirmed by other people's work. It may on the other hand merely be a suggestion for more fruitful experimenting.

The latter, for example, is the explanation of a significant dif-

ference that I found in my work in PK between the scores obtained in the morning and afternoon (8). I was not trying to prove anything; I was trying to find out how I could best work to get reliably high scores. As I said at the end of the article, I did not succeed in this task, but in the course of it I had an indication which was a good enough experimental guide that I might be scoring better in the morning than the afternoon. That nothing is proved by this should be obvious. Not from the fact that Mr Spencer Brown can, he says, do a random matching experiment of the same order of significance (2) but because the order of significance is such that the results I obtained might have been found by chance alone in 1 out of 90 occasions.

If Mr Spencer Brown's criticisms of ESP are irrelevant or erroneous, this does not dispose of the suspicion that ESP results may be statistical artifacts. That suspicion is, I think, widely held amongst many who know nothing of Mr Brown's work. They may be vaguely uneasy because they consider that successful ESP experiments are isolated and unrepeatable. This vague uneasiness is well expressed by Professor P. W. Bridgman in his contribution to the correspondence in *Science* which followed Price's original article. Bridgman finds it easier 'to think that [his] understanding of probability is faulty than believe in the genuineness of ESP,' and refers to ESP's 'utter failure to exhibit any regularities or to perform a single repeatable experiment'. We can, of course, argue against this last statement; the fact remains that belief in unrepeatability is a widespread reason for rejection of parapsychological findings amongst many who would otherwise be willing to give serious consideration to the evidence for ESP.

We cannot make a general rebuttal of attacks on parapsychology by saying that the evidence is stronger than would be regarded as necessary in any other science. In a sense it is, but the critics of parapsychology feel that the evidence is also different in kind from that in other sciences, and it is this difference in kind which I think is the real trouble between parapsychology and many of its critics.

Consider the position of a well-informed and sympathetic physicist who says: 'If there is anything in parapsychology it is of great scientific importance, but in a matter which is so unexpected and so important we must demand a higher level of evidence than we should in any matter that is more commonplace. And we have not yet got a sufficient level of evidence to carry complete conviction in this case.'

To the objection that parapsychology experiments are unre-

peatable and therefore not convincing to the scientist, there are, of course, several replies.

(1) One can say truly that the results of parapsychological experiments are much more repeatable than is commonly supposed. The findings of one experimentalist are often confirmed by another. We still fall short of the ideal of being able to specify the conditions that will give one complete certainty of what result we shall obtain. We have much less certainty than in a physical experiment and considerably less than in most psychological experiments. Greater repeatability may come in the future.

Few of us would feel any strong conviction of the reality of ESP if this rested on experiments that had never been repeated. If the Soal-Goldney experiment, with its astronomical odds against chance occurrence, stood alone, it would not generally create conviction of the reality of ESP. We should regard it as something very odd indeed; we might not be able to explain it. But if it stood alone, most of us would prefer to suppose that there was some other explanation (not known to us) than that the percipient had used a non-sensory channel of communication. Even those who accepted the ESP explanation would feel that so long as it was an isolated event, the truth of this explanation could not be held with strong conviction.

Why most of us regard this experiment as strong support of the ESP explanation is because it is not an isolated event. It confirms the earlier work of Rhine, Pratt, and others and has itself been confirmed by later work. It is not the small anti-chance odds alone but its position in this system of mutual reinforcement of work by different investigators that gives this set of experiments its evidential strength.

(2) Nevertheless, parapsychological experiments are not as repeatable as we should like them to be. As Rhine has said: 'The objective of the second stage [of parapsychology] is to bring the phenomenon to the point of production on demand, repeatedly and under specifiable conditions.' (5). Increase of the degree of repeatability of experimental results is at least one of the reasonable aims of parapsychological research. This is not because a high degree of repeatability is essential to the validation of parapsychological findings. This is sometimes said but, I think, mistakenly. It might prove to be the case that paranormal events were real but too erratic in the conditions of their occurrence for a high degree of repeatability to be achievable.

Yet if not essential, the maximizing of the degree of repeatability is obviously a convenience. Planning a research programme would be much easier if we could be reasonably sure of getting positive

results at will. There would also be the less important convenience that it would be easier to convince the sceptical experimental scientist that paranormal events do really take place. So I suggest that one of the aims that parapsychologists should set themselves is increase of the degree of repeatability of their experiments. The aim of being certain of what result will follow from a specified set of conditions may be unachievable, but we may hope to increase the probability of a specific parapsychological result following. I have already discussed some of the ways in which I hope this might be done.

(3) There is also a third thing that we should say about the question of repeatability to get it into the right perspective. Any scientific thinker may make his own criteria of the degree of repeatability necessary for his belief. He is justified in doing so; but saying 'I can't believe X' is not the same as saying 'X is not true'. It may be that a too stringent application of a rule of withholding belief from not completely repeatable experiments may prevent one from seeing a real fact of Nature.

But better than arguing is to develop a line of research to which such objections do not apply. The real test of a communication theory of *psi* is to find out whether you can use it to communicate something. A simple ideal *psi* communication experiment would be something like this. There would be two experimenters, E₁ and E₂, and a percipient P under the control of E₂. E₁ would have some information of which E₂ and P were ignorant. At the end of the experiment E₂ would, through P, have obtained the information by means of *psi* alone. The experiment might be a long one, perhaps taking many months. The information might to begin with be trivial. If however, real information got through under adequately controlled experimental conditions, then this result could not be explained by any form of a statistical artifact theory and the communication explanation of the ESP experimental results would be strongly confirmed. Also I think that, if the communication theory is true, then such conveyance of information should in principle be possible however difficult it may be in practice to carry out.

We must be clear what we mean by conveyance of information as distinct from an ordinary ESP result. Let us suppose that we have a subject who persistently scores high with an average of ten hits per run spread evenly over all symbols. All that we know because he has guessed *cross* on a particular occasion is something of a probabilistic order; it is no more than guidance as to reasonable betting odds. We do not know that the symbol on the card is a cross. We know that we could accept a bet of anything more

than 3 to 2 against it being a cross while we should require much longer odds against it being any other symbol.

Suppose that what was being guessed was not an ESP symbol but a series of letters conveying a message. So long as all one could say of any letter was that there was some probability that it was an *a*, some other probability that it was a *b*, and so on, one could not read the message. But when a sufficient number of these probabilities became so high that one could say with almost complete conviction that a certain letter was a *c* and another was an *h*, the point would soon be reached at which one could hope to read the message. This, of course, would be earlier than the point at which the separate letters could all be identified with complete certainty. The point at which the message could be correctly read would be the point at which one passed from the mere conveying of probabilities to the conveying of real information.

I think we can reasonably hope that the experimental study of *psi* can reach the point at which real information is conveyed by *psi*. In principle it seems to be possible, although there are many experimental difficulties to overcome. If we can succeed in this, it would be a very important breakthrough in parapsychology. There may be more than one way of achieving this result. The way I am at the moment backing is the repeated guessing technique which I am describing in a separate paper (not yet published).

In 1942 I had to give a presidential address to the S.P.R. I find that I said then: 'Let us now give up the task of trying to prove again to the satisfaction of the sceptical that the *psi* effect really exists.' That was seventeen years ago, but I have not essentially changed this opinion although I now think that I was over-optimistic about the extent to which the evidence for *psi* was enough to convince everybody. I think now that there is an irreducible scepticism; that is, irreducible in the present state of the evidence. I still think that what is required is not more accumulation of the sort of evidence we have now. What was done in the Pearce-Pratt experiments, the Pratt-Woodruff experiments, the Soal-Goldney experiments, was done for good and all. If someone else did it again, I do not think the volume of scepticism would be appreciably reduced.

If there is to be any reduction of this scepticism it must be by experimentation of a different kind. I have suggested three possibilities:

(1) The provision of a pool of successful scorers (not to be inhibited by experimenting to extinction). These to be available for sceptical scientists who wish to do a limited number of experiments under their own conditions.

(2) Renewed effort to discover methods of getting reliable ESP results. I think self-experimentation is the most hopeful line here. Certainly if we can succeed ourselves in producing good ESP scores we may not be equally successful in specifying the conditions in which other people will score. But I think it is the first step. Till we can make a good *psi* performance ourselves I do not think there is any hope of our discovering the good conditions for other people.

(3) I think a great deal depends on getting *psi* to work. The best hope I can see there is the repeated-guessing technique. I don't think it will be easy or quickly accomplished, but it looks as if there is here a possibility of getting ESP right out of the laboratory and doing a job of work, either of acquiring information or of conveying communication.

The few suggestions that I have made cover only a narrow range. The limitation with which I started (that I would think about the problem in the light of current criticisms) has meant that I have kept within the limits of card-guessing experiments or experiments derived from them. Anyone who knows the range of material covered by early observations in psychical research will realize how little of them has yet come under experimental method. We may look in the future to a much wider extension of experimental methods to the various problems in which the early psychical researchers were interested, including the problem of survival. One thing, for example, that all parapsychologists might do is to provide some experimental test of their own survival. This was attempted by Myers and by Lodge. I am also leaving my own cipher test in which I hope other parapsychologists will co-operate by finding out whether or not it is possible to obtain the key through a medium while I am alive and not wanting to transmit it (14).

I do not suppose that I have succeeded in the impossible task of making a true prediction of where psychology will go next. I may have done something useful if I have helped to direct the minds of younger and more active researchers towards this problem. I think it would generally be agreed that we have reached a stage in parapsychology at which the question of where we go next is becoming urgent. It seems clear that to go on going on as we have gone on in the past must lead to rapidly diminishing returns.

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THE RHODES EXPERIMENT LINKAGE IN EXTRA-SENSORY PERCEPTION

BY M. C. MARSH

REVIEWED BY G. W. FISK

THIS is the account of an investigation which was submitted as a dissertation and approved for the degree of Doctor of Philosophy of Rhodes University in the Department of Psychology. Rhodes University is at Grahamstown in the Union of South Africa. The report has not been printed but Dr Marsh has kindly sent us a bound volume of 450 foolscap sheets with a very full and detailed account of his experiments and including some 40 tables

and graphs. It will be placed on our Library shelves and deserves careful study by those interested.

Dr Marsh states the aim of the investigation was, first, to find whether 371 subjects, mainly students at Rhodes University, would be able to reproduce target drawings made 470 miles away by an agent in Cape Town, by means of G E S P¹; secondly, to find whether any association existed between any such hit scoring ability and personality ratings derived from several kinds of personality tests; and, thirdly, to find whether providing the subjects with material designed to link them more closely with the agent would increase their scoring rate.

17,440 drawings were returned by the 371 subjects. These were randomized and scored against a randomized set of 100 drawings consisting of 50 which had actually been used as targets, intermixed with 50 that were of equal difficulty but which had not been used as targets and which were inserted merely as controls. Three independent judges assessed the subjects' drawings and awarded hits in terms of title, shape and association.

How far were the three main aims of the investigation realized? First, in the crucial 'title hits' highly significant deviations from chance expectation in favour of the experimental target drawings were found; the control series of drawings showing no such effects.

Second, when the subjects were divided into a high-scoring and low-scoring group in terms of their ESP ability, the high-scoring group proved to be significantly more extraverted (as measured by the Bernreuter B31 scale) than the low-scoring group. In addition the data showed several other relationships and trends, predicted by the work of previous experimenters, but these did not reach the 0.01 level of statistical significance.

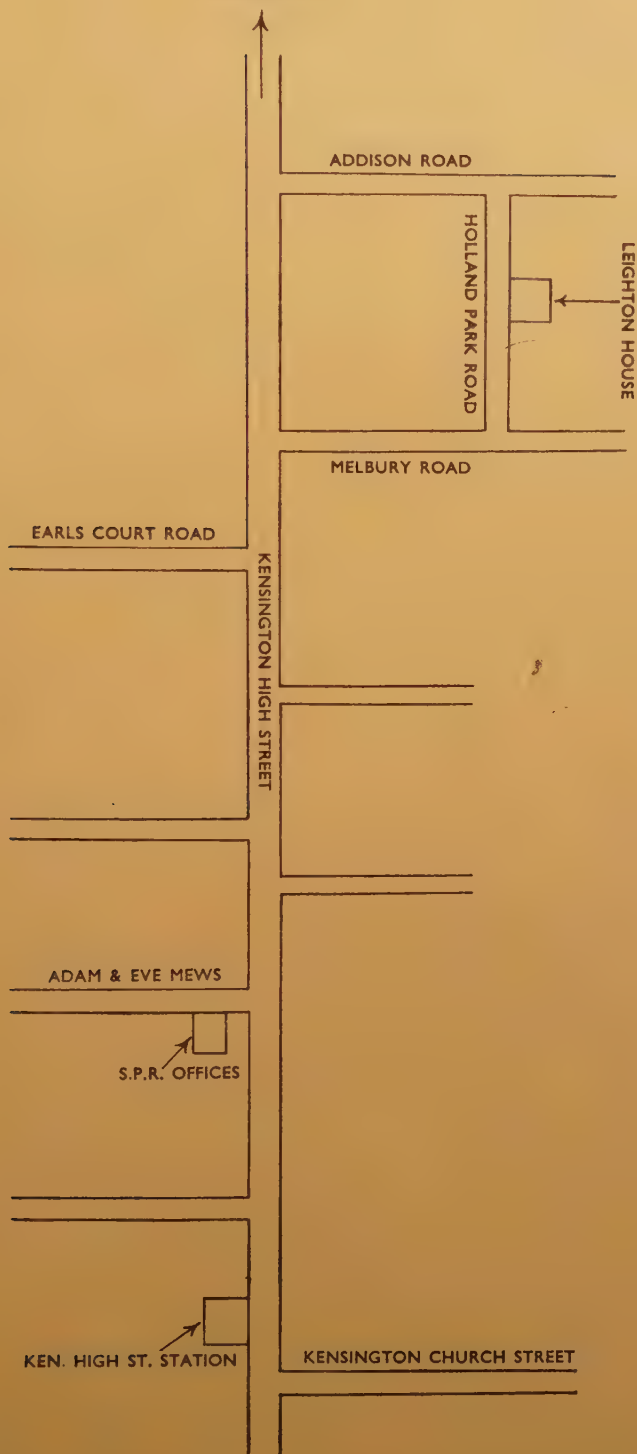
Third, to assess the effect of the Linkage Material the subjects were divided into an experimental and a control group. The former were supplied with correct Linkage Material, the latter with incorrect Linkage Material but which they were led to believe was correct. The two sets of Linkage Material were equated in all other respects. The Experimental Group of subjects showed significant improvements in scoring rates and, by contrast, no consistent significant fluctuations were shown by the Control Group subjects.

Thus the investigation may be said broadly to have succeeded in its three main aims.

Among some very interesting subsidiary effects observed may be mentioned the following:

¹ G E S P means General Extrasensory Perception: permitting either telepathy or clairvoyance or both to operate.

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LECTURE PROGRAMME

13th April—13th July 1960

All lectures will be held at Leighton House, 12 Holland Park Road, W.14, commencing at 7.00 p.m. on the dates indicated.

Wednesday, 13 April

DR IAN FLETCHER, M.R.C.S., L.R.C.P.
'HYPNOTISM—A RATIONAL APPROACH'

Dr Fletcher, who is a Member of Council, is Orthopaedic Consultant at Roehampton Hospital and has published articles and reports on Hypnotism in the Medical Press.

Wednesday, 11 May

DR JOHN SMYTHIES, M.A., M.D., M.Sc., D.P.M.
'NEUROLOGY, PHILOSOPHY AND PSYCHICAL RESEARCH'

Dr Smythies is a Member of Council, is a Senior Registrar at Maudsley Hospital, has a distinguished record for scientific research and is author of 'Analysis of Perception'.

Wednesday, 15 June

'QUESTIONS AND ANSWERS'

At this meeting a panel of Council Members will deal with questions on Psychical Research which members are invited to submit in writing to the Hon. Secretary before May 1st 1960. Time may not allow all questions submitted to be answered, in which case they will be selected to give a representative coverage.

Wednesday, 13 July

ALAN GAULD, M.A.
ALBERT HUNT MEMORIAL LECTURE
'THE SUPER-E.S.P. HYPOTHESIS'

Mr Alan Gauld, Emanuel College, Cambridge, has kindly agreed to deliver this first lecture in the above memorial programme.

[P.T.O.]

Any Member wishing to bring a guest to any of above
Meetings should apply to the Secretary General.

It was found that the subjects' hits were distributed evenly throughout the whole 25-day period of the experiment on each target, and showed no tendency to occur more frequently in the week a particular drawing was being used as a target than in the other weeks when it was not. This, as will be seen when later I describe the methods used by the agent to choose and display the targets, would *prima facie* seem to require both pre- and post-cognitive powers on the part of the subjects.

A qualitative examination of the hits scored by the subjects revealed that they tended on the whole to reproduce the concepts depicted by the target drawings rather than the actual shapes drawn by the agent. Dr Marsh remarks that this suggests that the hits were being produced by a telepathic process rather than by clairvoyance—that the subjects were assisted by the Linkage Material to gain access to the agent's mind as a whole, and not to particular items in it. Dr Marsh goes on to say that this throws doubt on the correctness of Whately Carington's Association Theory of Telepathy.

In his Introduction Dr Marsh gives a short history of the development of Psychical Research from the first interest in spontaneous cases of unexplained paranormal experience to the later use of quantitative methods of evaluation. He also surveys in some detail the chief sources of inspiration for this Rhodes experiment, in particular the work of Hettinger, Whately Carington, and, in the personality field, of Dr Betty Nicol, Stuart, Schmeidler and others.

The 371 subjects were nearly all students in residence at Rhodes University College; the few exceptions were either members of the staff or interested outsiders. The age range was from 16 to 63 years, the great majority in the 17 to 21 year old classes. All subjects had been previously 'conditioned' by lectures on ESP, interviews etc. As a matter of policy this personal contact was maintained as much as possible throughout the experiment.

Before the main experiment started, a battery of personality tests was administered to the students. The tests chosen were:

The Washburne Social Adjustment Inventory, Thaspic Edition.

Pressey's X-O Tests.

Allport Vernon Study of Values.

Bernreuter's Personality Inventory.

Samples of the forms used in these tests are found in an appendix.

After the subjects had completed their main trials they were

asked to complete a Psychic Questionnaire. This questionnaire was a particularly searching enquiry and included the questions asked by the compilers of the S.P.R. Census of Hallucinations.

The choice of the Target Drawings

It will be remembered that Whately Carington selected his originals by opening a dictionary at random and making a drawing of the first object on that page that could be drawn. Dr Marsh considered that this method was open to a number of objections. The most serious of these was that there was no control over the 'popularity' of the originals so chosen. Hence Carington might have a large chance factor entering in to dilute his results. Thus if the choice of an original happened to fall on 'car' large numbers of his subjects might be expected to draw cars from chance alone and this chance scoring would tend to obscure any genuine ESP.

Carington also compiled a catalogue of the frequencies with which the various drawings had occurred in his experiments and used the catalogue to establish the chance frequency with which any object could be expected to occur. But Dr Marsh considered it unjustifiable to assume that subjects drawn from one culture would tend to think of objects with the same frequency as subjects from a different culture.

So Dr Marsh decided that more valid frequencies would be obtained if the mental content of the actual subjects who were going to take part in the experiment was sampled just prior to the start. Accordingly all the volunteers were asked to make a list of 50 objects which, in their opinion, could be drawn easily and unmistakably. The lists were then collected and analysed and the frequency of each object determined. The lists produced 15,828 titles, 992 of which were 'singles' (i.e. a title suggested by one volunteer only). From these 'singles' the attempt was made to compile a list of 100 titles as unlike one another as possible, so that no confusion of shape would arise. From these 100 titles the 50 Experimental Target Originals and the 50 Control Series Originals were selected by a random method. On a recheck it was found that 7 of the reputed 'singles' were actually 'doubles' which initial inequality had to be allowed for in the subsequent assessments.

Time sequence of the Experiment

The experiment was divided into five weekly sections, A, B, C, D, & E, of five days each, 25 experimental days in all. Two drawings were made by the agent each day, 50 drawings in all.

Procedure followed by the Agent in Cape Town

The titles of the 100 selected possibilities were printed on one inch squares of cardboard. These were put into a wooden selector box, which was completely enclosed and had a slit only large enough to allow one of the cards to be shaken out at a time. This box was sealed and sent to the agent. On the first day of the experiment he broke the seal over the slit, shook two cards from the box and made two simple outline drawings of the objects so selected. In all, 50 such drawings were made by the agent leaving 50 unused cards still in the box which had not been seen by him. Some months later someone unconnected with the experiment was asked to make drawings of these titles similar to the drawings already made by the agent. These were the Control Series Originals.

It appears that only one person, the agent, saw or handled the drawing originals till after the experiment. To quote from his statement:

Each morning between 7.10 and 7.15 a.m. I shook out two squares of cardboard from the selector box . . . I then got out of bed, took two clean sheets of paper from those supplied . . . and made a rough pencil drawing of the objects (one on each sheet). When satisfied I went over the outline with Indian ink.

I then removed the drawings made the previous day from an exposure box [also provided by the experimenter], and in their place put the two new drawings. I placed the two drawings from the previous day in an envelope and locked it away in my cupboard. The two squares of cardboard I put in another envelope and also locked it away. I locked the box with the two drawings for the day and kept the key, together with the key of my cupboard, in my pocket throughout the day. . . .

I took special care not to mention to anyone that I was even doing the experiment and certainly never mentioned what the particular drawings for the day were. I did not even tell the members of the household that I was doing the experiment. . . . In addition to all these precautions I purposely refrained from writing to the experimenter while the experiment was actually being carried out.

It will be noted that there was no 'display' of the two targets for the day by placing them in sight of the agent for the 24 hours. On the contrary after the drawings had been made, they were immediately locked up for the rest of the day and removed next morning, placed in envelopes, and stored away in a locked cupboard. It is difficult to see what object was served in first locking them in the 'exposure' box at all. After the first few minutes of preparation it is unlikely that the agent thought much about them

at all during the rest of the day. It was probably a case of 'out of sight out of mind'. It must also be remembered that some persons had previously prepared the 100 cards with the title of each original target, experimental or control, printed on them. Moreover a number of the 371 subjects had themselves imagined and drawn one of the 100 targets. Hence if the results favour a telepathic hypothesis it is not certain that only the agent's mind was involved. If a clairvoyant hypothesis is preferred then it should be remembered that all the 100 titles cards were, at all times, either locked in the cupboard, or were in the target box, or in the selector box; not only the experimental target cards but the control cards as well. This should be borne in mind when, later, we shall see that the percipients' scores, with some of the varieties of hits assessed, were disconcertingly considerably higher on the control drawings than on the actual experimental ones!

Procedure followed by the Experimenter & Subjects in Grahamstown

On the Sunday just prior to the beginning of each five-day section, each subject was issued with a book in which the ten drawings for the week were to be made, together with the general and specific instructions they were to follow, and any linkage material. On the Saturday the books were collected from their rooms. As a matter of policy the experimenter also tried to see as many of the subjects as possible personally each week to keep up their interest. Since they were never told what the experimental originals were, nor their success or failure, it was easy for their interest to flag. 17,440 drawings were returned out of a possible 18,550, which is a 94% return.

The use of Control and Experimental Groups of Subjects

The subjects were divided into two groups approximately equal in age, sex, etc. After the first week the two groups were told they would not have the same agent but would each have a different one also living in Cape Town. They were never told there was only one agent and that one group was being used merely as a control.

Both groups were given precisely similar instructions but whereas the Experimental Group was given correct linkage material that given to the Control Group was incorrect. No linkage material was provided for the first five-day section in order that this section might provide an indication of the normal scoring level of the two groups when no linkage was operating and whether the selection method had succeeded in equating the two groups in terms of ESP ability.

The linkage factors introduced in the five sections

A. First section. None.

B. Second section.

Similar squares of five different colours were pasted on the agent's and subjects' drawing papers; a different colour for each day. Wrong colours were given to the Control Group. The agent was asked, after he had made his drawing to associate it with the square of colour by trying to imagine what the drawing would look like if it had been made in the same coloured ink.

C. Third section.

A number of handkerchiefs were obtained from the agent which he had been using for a number of months. These were cut into small one inch squares and one given to each of the subjects, who were told to hold them in their hands when making their drawings. Similar squares were given to the Control group but which were the property of someone wholly unconnected with the experiment.

D. Fourth section.

The Experimental group subjects were given an actual photograph of their agent taken in his room at Cape Town, sitting next to the exposure box into which he put the targets drawings each day. In addition they were told some of his personality test results (the same tests as the subjects had undergone) and also supplied with a friendly letter written by the agent telling them some of his intimate personal history. The Control group were given material quantitatively and qualitatively equal to that received by the Experimental Group but the photograph was of a man unconnected with the experiment and the personality tests results were fictitious and belonged to no living person.

E. Fifth section.

The agent repeated his procedure of Section B (colour squares) and the information booklets used in D were returned to the subjects. (No colour squares were put on the subjects' drawing sheets.) In addition the subjects were also given their own personality tests results and asked to compare them with those of the agent.

After Section E trials had been completed the subjects were asked to fill in and return the Psychic Questionnaire. That constituted the last stage of the experimental procedure, and after it no further data was obtained.

How the data were scored

Precautions were taken to ensure that the three judges had no means of knowing whether any particular drawing was made by an experimental or control subject. The judges compared each of the 17,440 subjects' drawings with each of the 100 originals, each judge working independently, and classified any resemblances according to the following scheme:

The resemblances were expected to be of three kinds:

- (a) The NAME or title of a percipient's drawing may resemble the name or title of the original.
- (b) The SHAPE or content may show resemblance.
- (c) The IDEA expressed by a percipient's drawing may reproduce the idea expressed by the original although no resemblance (a) or (b) can be allocated.

Assessing each of these three factors independently the following code was to be used for the records:

- (a) NAME
 - A—Name wholly right ('Fountain' for 'Fountain').
 - B—Name generalized ('Mug' for 'Beer Mug').
 - C—Name specialized ('Beer Mug' for 'Mug').
 - D—General name right, but different species ('Beer Mug' for 'Tooth Mug').
 - E—Synonym ('Car' for 'Automobile').
- (b) CONTENT of Drawing (actual shape).
 - F—Original reproduced without additions or omissions.
 - G—Part given, whole reproduced (whole figure drawn when original showed face only).
 - H—Whole given, part reproduced (only finger drawn when target was whole hand).
 - Add if distorted: x
 - Add if elaborated: o

Indicate degree of resemblance thus:

 - 3—good
 - 2—fair
 - 1—faint
- (c) IDEA or ASSOCIATION
 - I—Clear association though name and shape different ('Wind' for 'Weather Vane').
 - J—Idea right, name wrong ('Chrysalis of Man' for 'Mummy').
 - K—Miscellaneous resemblances not falling under any other type of hit.

After all three judges had completed their assessments, the judgments of all three were combined by accepting the majority decision or by accepting the lowest of the assessments if all three judges disagreed.

Examples : AF_3 AF_3 $AF_3 = AF_3$
 AF_3 AF_2 $AF_2 = AF_2$
 AF_3 AF_2 $AF_1 = AF_1$

In cases where there was total disagreement on any type of title or association hits an arbitrary decision had to be taken by the first judge. This occurred very seldom. As an additional safeguard Mrs Forster (of Duke University) undertook to review all the A hits.

Analyses of the Data

Dr Marsh proceeds to give five main analyses of the data in very considerable detail. There are also discussions as to the legitimacy of the statistical procedures employed. In this Dr Marsh had help from members of the Parapsychology Laboratory of Duke University, particularly from Drs Greville and Pratt. I can do no more here than summarize the more salient results of the analyses.

First Main Analysis

Differences in the frequencies of hits scored on Experimental and Control Series Originals.

TABLE I (P. 107)

Summary of Calculations C1 to C20

Type of Hit	Hits on Exp. originals	Hits on Controls	Difference	Chi square totals
A	133	37	+ 96	54.76
B	335	342	- 7	0.074
B + C	337	350	- 13	0.26
D	114	180	- 66	15.08
E	32	126	- 94	56.42
F ₃	151	52	+ 99	48.84
F ₂	390	212	+ 178	54.50
F ₁	595	329	+ 266	80.86
G ₁	22	11	+ 11	3.66
G ₁ + G ₂ + G ₃	28	11	+ 17	7.44
H ₃	80	29	+ 51	24.00
H ₂	220	330	- 110	22.72
H ₁	356	434	- 78	8.06
I	689	570	+ 119	12.12
J	133	258	- 125	40.88
K	57	39	+ 18	3.40
ALL HITS	2,770	2,419	+ 351	33.80

A plus sign before a deviation indicates that more hits were scored on the Experimental Series than on the Control Series.

All Chi square totals are for one degree of freedom.

As a guide to the probability values of the Chi square totals:

$\chi^2 = 3.84 =$	a P value of approx. 0.05
5.41	0.02
6.64	0.01
10.83	0.001

(Dr Pratt calculated the P value of 54.76 as less than 10^{-12}). It will be seen that hits of Types B, B + C, G1 & K yield differences too small to be regarded as significant at the 0.01 level. In Types D, E, H2 & J the judges awarded more hits on the Control Series Originals than on the Experimental ones, the differences being significant at the 0.01 level or better. This, to me, seems a very surprising result. It should be noted that these are types of hits covering fainter resemblances in shape, title or association. Did the experimenter carry his classification of the types of hits too far for usefulness and the allocation of 'marginal' hits obscure the outcome of the trials?

For Types A, F3, F2, F1, G1 G2 G3, H3, I, and all types of hits combined, which include all the 'strict judgment' categories, the subjects have scored very significantly more hits on the Experimental targets than on the Controls.

So the first general conclusion that emerges from a study of these figures is that some factor or factors were operating that caused the subjects to produce highly significant different scoring rates on the two different series of originals.

Moreover it is clear that this significant result could not be due to large score contributions from a few individuals in the groups. Thus the 133 A hits on the Experimental Originals were scored by 106 subjects:

2 subjects scored 3 A hits each = 6 hits					
22	"	"	2	"	" = 44 "
82	"	"	1	"	" = 82 "
<hr/>			<hr/>		
106			132		

(There is a slight discrepancy here. Total A hits thus analysed = 132. 133 is the correct number as given in Table 1).

The report then goes on to discuss in considerable detail what factors might account for the observed differences, under the following heads:

- (a) Factors pertaining to the method of selection of the originals.
- (b) Deviations by the agent from his instructions.
- (c) Unconscious selection of favourable originals due to use of PK and precognitive ESP. (Straining at the gnat of ESP and swallowing the camel of PK!)
- (d) Factors relating to the subjects which might influence the frequency with which certain originals came to mind during the experiment.
- (e) Rational inference and/or Sensory leakage.
- (f) Possible bias arising from the ability of the judges to distinguish between the Experimental and Control originals.
- (g) Possibility of judges adopting too lenient a standard in awarding A hits.

After examination all the factors under the above headings were dismissed as possible influences on the scores with the exceptions of (d) and (g).

Regarding (d) there might be outside factors, beyond the experimenter's control, that might influence the subjects' choice of drawings—transient chance factors that might leave their mark on the data in the form of atypical spurts or deficits of hits in certain sections of the experiment leading to what was called a 'Bunching Effect'.

Thus, as it turned out, there was at one period a failure in the town's electricity causing a sudden demand for other means of illumination and resulting in the subjects drawing a large number of candles and candlesticks in one particular week, but not in other weeks. Altogether sixteen possible cases of bunching were discovered and in many of these it was possible to relate them to some event in the environment of the subjects.

Consequently the experimenter prepared 'fully corrected hits' to take the place of the raw data originally considered. Thus only one hit on 'candlestick' was allowed to stand—all the rest of the hits on this original were eliminated for safety. The one hit was allowed to stand, since it would be expected on the basis of chance. Altogether such eliminations were carried out on 12 suspected 'bunches'. This, of course, was a very drastic procedure and may well have eliminated some genuine ESP hits, but the experimenter concluded it had to be done.

Regarding (g) the possibility of too great a leniency in the standards followed by the judges Dr Marsh obtained the help of Mrs Forster (of Duke) to re-examine all the 170 A hits allowed by the primary judges. She adopted a most stringent standard and

rejected 21 A hits in all placing them in lower classes. Among the A hits rejected by Mrs Forster were:

Flitgun for *Flit Spray*
Laurel Wreath for *Laurel Crown*
Egg Timer for *Hourglass*
Triumphal Arch for *Arc de Triomphe* (!)
Mitre for *Bishop's Mitre* etc.

which goes to show how meticulous and rigid was Mrs Forster's judgment. So, after discarding the doubtful A hits allowed by the three primary judges; also discarding six A hits that were scored by persons who had originally suggested the titles; discarding all but one of the hits on the 'bunches', the balance of A hits did not seem open to further criticism, and were called 'Fully Corrected A Hits'.

A new calculation then produced a revised value for the Chi-square of the differences in the frequencies of hits scored on the Experimental and Control Series Originals (C21A on p. 134 et seq.)

E.S.O.	Observed hits	89	C.S.O.	27
	Expected	60.71		55.29
	Deviation	28.29		-28.29
		$\chi=27.65$		

as against $\chi=54.76$ (p. 11) for the raw A hits. A considerable drop but still highly significant.

After lengthy and detailed discussion Dr Marsh claims that the results of this First Main Analysis are consistent with the telepathy hypothesis. But he is properly cautious. To quote his own words:

There always remains the possibility that the writer has overlooked some factor or factors which would account for the results normally. Consequently the conclusion is stated tentatively that the data suggests that the subjects were in fact using ESP to score hits on the Experimental Series Originals. If subsequent analyses reveal other secondary effects of an extra-chance nature, then the deviations discussed above can safely be attributed to the operation of ESP.

Second Main Analysis

This was an examination of the data to find whether the subjects tended to score significantly more hits on the Experimental Series Originals during the week in which they were targets than in other weeks of the experiment.

Separate calculations were made on the Raw Scores and on the Corrected Scores. In both cases the differences tested failed to reach the 0.01 level of significance. It shows that the subjects' hits scored on any particular original were scattered more or less evenly, over the whole experimental period. It is interesting to note that Carington also found much the same effect when he carried out his experiments IVa and IVb (*Proceedings* 43, 46).

Third Main Analysis

The relationship of hit-scoring ability to personality factors. This analysis takes up almost one-quarter (100 pages) of the whole report. After a review of previous research findings on the relationship of personality differences to ESP ability the experimenter ventured to make five main predictions of what the Rhodes data would confirm. It is not stated on what date these predictions were formulated, but it seems clear it was before the Third Main Analysis was performed. The analyses took many months of work by several helpers and the Duke Parapsychology Laboratory gave financial assistance as well as advice. I now quote the predictions together with the later findings bearing on them.

PREDICTION NO. 1

If the subjects are divided into a high-scoring group, and a low-scoring group, the high-scoring subjects will be found to be more extraverted in terms of the Bernreuter B31 scores than the low-scoring group.

Findings: Assessment by the mean-difference method showed the high scorers to be significantly more extraverted. P lies between 0.0005 and 0.0003. The median test confirmed this trend at a lower order of significance, P dropping to between 0.05 and 0.025.

PREDICTION NO. 2

The high-scoring subjects will be better adjusted than the low-scoring group.

Findings: Three possible measures of adjustment were assessed. With the Washburne Subtotal assessment the mean difference showed the predicted trend but P only reached the 0.25 level of significance. (But when the assessment was made 'in terms of Q ' the trend was reversed.)

Using the Bernreuter F1-C measure the scores showed the predicted trend in both mean difference and median test assessments. P reaching 0.0244 and 0.025 to 0.01 respectively. Using

the Bernreuter B₃-1 scores both mean and median differences showed the same trend. $P = 0.0005$ to 0.0003 and 0.05 to 0.025 respectively.

PREDICTION NO. 3

The 'sheep' should score more hits than the 'goats' though the difference can be expected to be relatively small.

Findings: The sheep scored $0.008,301$ hits per trial. The goats $0.006,076$. Difference $0.002,225$ h.p.t. P lies between 0.15 and 0.10 . Thus there is a suggestion that the trend is there.

PREDICTION NO. 4

If sheep and goats are further classified as 'well-adjusted' and 'poorly-adjusted' the former should score more hits than the latter.

Findings: There were signs of the predicted trend but they failed to reach the 0.1 level of significance. The 'well-adjusted' sheep scored $0.008,102$ hits per trial. The 'well-adjusted' goats $0.007,463$ h.p.t.

PREDICTION NO. 5

The differences in scoring rates between the 'well-adjusted' sheep and goats will be greater than that between the sheep and goat groups as a whole.

Findings: The predicted trend was not shown in the Rhodes results. Thus, to sum up, in the case of A hits, the Rhodes data shows four out of the five predicted trends. In the case of those trends reaching statistical significance a number of alternatives to the ESP hypothesis were considered but were found inadequate to account for the results. Unless one or more counter-hypotheses have been overlooked these significant personality relationships which have been found must be taken as establishing that ESP on the part of the subjects has been a factor in producing the A hits thus removing any ambiguity in the interpretation of the results of the First Main Analysis. The concluding section of the Third Main Analysis is devoted to a comparison of certain answers to the Psychic Questionnaire returned by a group scoring 2 or more hits with answers returned by a group scoring 0 hits, to discover if any differences revealed are statistically significant.

The Questionnaire sought for answers to over 80 questions under the headings of General, Imagery, Personality traits, Religious attitude and Psychic experiences and attitude.

Although the differences observed between the good and bad subjects are suggestive and interesting none reached a statistically significant level, probably because the number of subjects con-

cerned in particular differences was too small. The most promising question was the one sampling sleep-talking experiences as a child. Of the ESP group, only 4.55% had not talked in their sleep in childhood, as against 24.68% in the non-ESP group.

Experience of sleep-talking in adults showed a less marked difference: 13.04% as against 31.12%. P here lay between 0.2 and 0.1. Of the ESP group 70.83% dream frequently as against 47.21% of the non-ESP group. Again P lies between 0.1 and 0.2.

Only 25% of the ESP group attended church frequently as against 45% of the non-ESP group. (Why should ESP ability tend towards agnosticism?) Experience of Déjà vu: 85% of the ESP group experienced this phenomenon as against only 68% in the non-ESP group. Of the promising differences for which no assessment of significance could be obtained the following deserve mention:

- (a) The ESP group were more aware of 'atmosphere'.
- (b) As adults they sleep-talked more frequently.
- (c) They accepted the reality of psychic phenomena more frequently.
- (d) They more often experienced hallucinations than did the non-ESPs.

It should however be emphasized that none of the differences assessed reached the 0.05 level of significance.

Although the data may suggest the relationships outlined above it by no means establishes them. More investigation is required with larger groups of subjects.

Fourth Main Analysis

Linkage. Perhaps the most interesting problem confronting parapsychologists today is the nature of the relationship between subject and agent in a psi experiment. This problem Dr Marsh calls the Linkage Problem and the whole report is entitled 'Linkage in ESP'.

It is as well to bear in mind that the problem may possibly be in one sense an unreal one. It is true the brains of the agent and the subject are separated in space, leading to the feeling that there is a gap in some way to be bridged before any psi action can take place. But Professor Broad has shown that brain to brain communication is only one of a number of possible hypotheses. (See his paper 'Normal Cognition, Clairvoyance and Telepathy,' *Proceedings* 43, pp. 397-438.) The relationship might be mind to mind and the assumption that minds have spatial qualities may not be valid. Some psychologists have also employed such concepts as the Group Mind and the Racial Unconscious that imply a

mental organisation that transcends the minds of individuals. Hence any research that helps to throw any light on these questions is, as Dr Marsh considers, of great importance.

I have detailed above (see p. 225) the linkage factors that were introduced into section B, C, D & E of the experiment. The effects of the introduction of these factors have been analysed as completely as the data would allow and illustrated by numerous tables and graphs. Here I can do no more than summarize the main conclusions to be drawn.

(a) It was found that no significant difference in hit scoring ability existed between the Experimental and Control Group of subjects before the provision of linkage material (i.e. in the first week).

(b) When the subjects were provided with a photograph of their agent and a description of his personality consistent significant improvements in their scoring rates took place. Provision of an equated but incorrect set of linkage material to the Control Group yielded no such significant improvement.

(c) Provision of 'psychometric' handkerchief links produced two significant increases but neither so consistent nor significant as in the case of the photograph. Incorrect handkerchief links given to the Control group produced no such increases.

(d) Provision of associative links in the form of colours caused no significant increases in scores for either subject group but unexpectedly did produce a highly significant and fairly consistent *decrease* in the hits scored by the Experiental Group on the Control targets. By contrast the Control Group, who received incorrect colour links, showed no such decrease.

Dr Marsh suggests that the correct colours acted by guiding the subjects away from the originals totally unassociated with any squares of colour. Some of the subjects complained they had found the colours distracting and hindered them getting impressions. It is likely they would feel an urge to draw objects that could be associated with the colours—a pillar-box for red, a boat or plane for the green or blue, etc. It is obvious that further problems concerning the linkage process have been raised by this experiment, *inter alia*: the number and relative importance of various factors such as the correctness of the linkage, its suggestion value, the decline effect, and so on; whether the effect exerted by the linkage is cumulative or not, and many others. Specially designed experiments are needed to obtain answers to such questions.

The Fifth Main Analysis

A qualitative evaluation of a sample of the drawings returned by the subjects, to complement, to some extent at least, the more abstract assessments of the statistical significance of the previous analyses. There are in this section some fifty pages of reproductions of the actual drawings of the targets and the subjects' attempts. The following are the main conclusions drawn from the examination:

(1) Very few, if any, of the good shape resemblances are photographic likenesses of the target originals.

(2) The best shape resemblances occur when the target is a very simple object of conventional design.

(3) When the agent deviates from the conventionally accepted pattern of the object, very few of the subjects reproduce the deviations in their drawings. But occasionally there are, more or less, fumbling attempts to do so which seem to me to be very instructive. To illustrate this I include in this summary a copy of the drawing of the target for 'Saturn' and the seven hits made by the subjects. The first point of interest is that Saturn was used as a target only in the last week E, actually on the penultimate day of that series. Hence all the hits on it were precognitive (though it must be remembered that the card bearing the word Saturn had been, of course, in the Selection Box mixed with a diminishing number of the others during the whole course of the experiment).

Now to me it seems almost incredible that the agent (about 21 years of age, a former scholar at Rhodes and experienced in two different Government offices) reports that when the title Saturn appeared out of the selector box, he found he had no idea of the appearance of Saturn and had to refer to a dictionary. (And this is the age of Space Travel fiction!) He learned that it was a planet with two rings and nine moons. When he drew it, he placed the rings co-axially but not in the same plane. He spattered the nine moons in a random fashion producing a highly original and distinctive version of Saturn differing markedly from the conventional idea of the planet. The nine moons seem to have been too much for all the subjects for they have been omitted by all of them. Four of the subjects produced conventional drawings of Saturn (11512, 10420, 4443, & 9658) with one ring, entitled 'Saturn', 'Saturn', 'Moon' and 'World'. The other three appear to be groping towards the concept to be drawn later by the agent. Suggestions of a second ring are visible perhaps in 6196. In 15645 the subject has sketched a first ring with confidence, and then hesitantly dotted in a second ring above it, in the correct

position shown by the target. Significantly he has refrained from calling his reproduction Saturn but been contented with 'Planet'. Was this because it went counter to the subject's knowledge of the appearance of Saturn?

The last example, 15716, is also interesting. It is as if the subject had eked out his drawing from fragmentary communications from the agent—as if the agent had said: 'I am drawing Saturn . . . it has two rings in different positions round it . . . and then the communication channel broke down and the subject had to make the best drawing he could from the scanty information received. He struggled with the position of the rings, made a second attempt, and, because his drawing did violence to his concept of Saturn he labelled it 'Planet' with an added interrogation mark.

The 3 formulae AH_3 , H_3J etc. under each drawing are the scores allotted by the three judges.

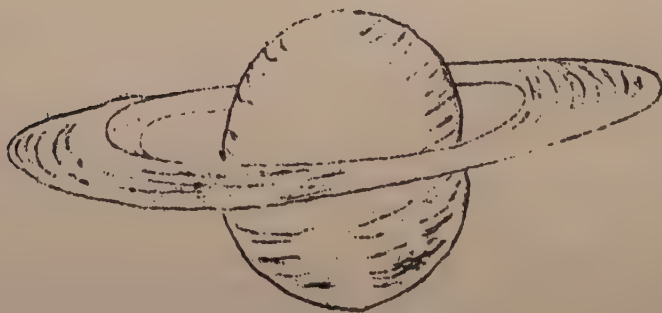
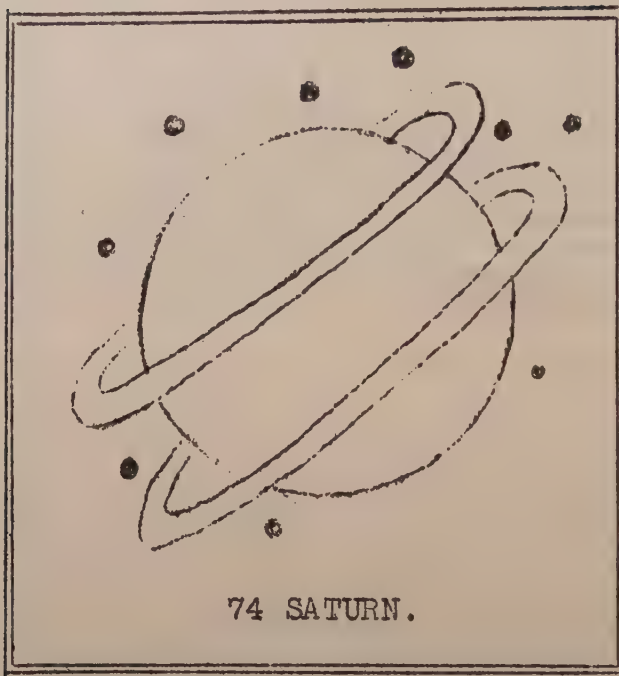
Dr Marsh states that it would seem justifiable to say that the evidence as a whole refutes the idea that the subjects were directly apprehending the drawings in a clairvoyant way, though personally I think the point is debatable. But Dr Marsh considers the subjects' impressions did not come through anything akin to a mental image. He rather thinks that in this experiment ESP depended on the transmission of concepts (akin to verbal or ideational concepts) usually of a fragmentary nature. This would confirm some of the findings of Warcollier.

Everyone will agree as to the fitful and unreliable nature of the ESP process. What constitutes the most successful types of targets? A fair proportion of the targets had no A hits scored on them whatever. Can any reader find a common element in the following list of targets on which there were no A successes? Morse Key, Flit spray, Turnstile, Javelin, Pendulum, Asterisk, Barbed Wire Barb, U-tube, Sentry box, Wing nut, Honeycomb, Cricket pads, Taj Mahal and Rhinoceros?

In conclusion I will quote from Dr Marsh's own final reflections on the results of his research. He remarks that

during the course of the assessments made in this investigation each new fact revealed has been carefully considered, and an explanation sought for it in terms of normal and paranormal causes. As more and more facts accumulated from the various analyses, a gestalt pattern began to emerge which was logical and consistent with the ESP hypothesis, but not with any of the alternative hypotheses that came to mind. This pleasing logical consistency of the results is, to the writer's way of thinking a powerful additional argument for the acceptance of the ESP and Linkage Hypotheses over and above the mere

statements of statistical probability. Part of the difficulty confronting a parapsychologist is that the occurrence of ESP can only be established by eliminating all counter hypotheses, and there always remains the possibility, however slight, that a normal explanation capable of accounting for the results has been overlooked. To deny the existence of such a possibility is to commit the sin of hubris. . . . One of the aesthetically pleasing rewards from undertaking this research has been to see this meaningful pattern emerge. . . . This is not to say that the data has always fulfilled *a priori* expectations, but it has shown a number of the trends previously established as being typical of ESP data. The results of the Rhodes Experiment thus offer strong evidence for the existence of ESP and the effectiveness of certain linkage procedures. They also confirm many of the reported findings of previous investigators.



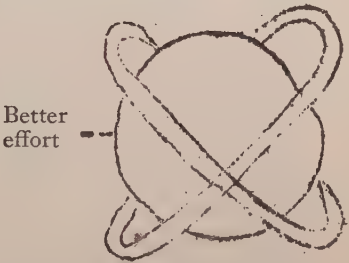
11512 'Saturn' AH_3 , AH_3 , AH_3



10420 'Saturn' AH_3 , AH_3 , AH_3

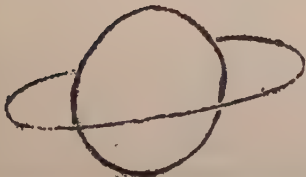


4443 'Moon' H_3J , H_3J , H_3

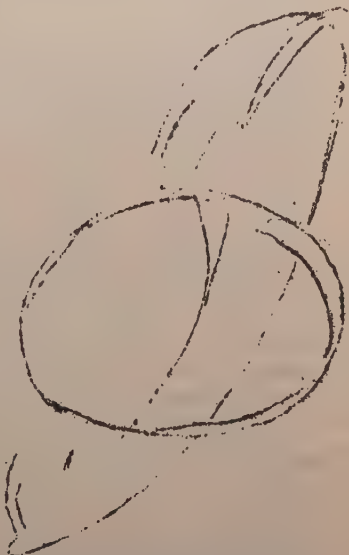


Better effort

15716 'Planet' EFx_3 , BFx_3 , BHo_3



9658 'World' BH_3 , H_3 , CH_3



15645 'Planet' EHx_3 , BH_2 , BH_3

6196 'Saturn' AH_3 , AH_3 , AH_3

A RE-EXAMINATION OF CERTAIN
STEWART DATA

BY S. G. SOAL

I. PLAYING CARD EXPERIMENTS

IN *Modern Experiments in Telepathy*, pp. 295-99, the authors discussed some experiments carried out between 14 March and 30 May, 1949, in which ordinary playing cards were employed instead of five symbol cards. We reached the conclusion that while there was a highly significant score of 78 hits that were completely correct in the series of 2,000 trials, there was no evidence whatever that the percipient was sometimes partially successful in getting the colour, suit or value correct. That is to say Mrs Stewart apparently cognized the card as a unitary object or not at all. Unfortunately the section of our book was not written until over three years had elapsed and I stated, I now am sure incorrectly, that Fisher's tables of two figure random numbers were employed to construct the target sequence. At the time of the playing card experiments I had been using these tables a good deal for quite another purpose and this may have caused some confusion in my memory after the lapse of time. Actually I find there is no mention of Fisher's tables on the scoring sheets themselves or, indeed, in the chronicle.

It is quite clear from even a cursory examination of each of the ten experiments of 200 trials that the 52 target symbols are not distributed in a Poisson distribution as one might expect to be the case had the lists been compiled from random-number tables and this was first pointed out to me by Dr Pratt of Duke University two years ago. But this does not necessarily invalidate the conclusions drawn from the data or mean that the lists were made up in somebody's head. That *some* method of randomization was used is I think obvious from the consistency which the data exhibit and the very orderly behaviour of an extensive series of cross-checks as regards both expectation and variance. Bernoulli distributions of targets are not the only ones that can be used in card-guessing experiments; Dr Rhine's packs of Zener cards are an example in point. Or again, if we mix thoroughly four packs of ordinary playing cards to form a sequence of 208 trials it is clear that since every card occurs exactly four times the odds against the target cards forming a Poisson distribution would not be 10^4 or even 10^6 to 1 but 10^∞ to 1. Yet such a target sequence would be admirably adapted for use in a card-guessing experiment. The

mean expectation of hits is 4 and the *maximum* variance can be shown by W. L. Stevens' formula to be 4.019, i.e. $4 \times 20/207$. If the cards are properly mixed we may work out expectation and variance from the number of times each target-symbol occurs and the number of times it is guessed.

Stevens' Expectation and Variance

I shall denote the 10 experiments of 200 trials by the letters A-J in chronological order. In Table I I have worked out by Stevens' formula the expected numbers of hits and the variance for each set of 200 with respect to card, colour, suit and value. It will be seen that the figures are remarkably consistent.

TABLE I

	Card		Colour		Suit		Value	
Sitting	E_S	V_S	E_S	V_S	E_S	V_S	E_S	V_S
A	3.61	3.52	100.00	50.01	49.57	37.08	15.01	13.73
B	4.06	3.95	100.00	50.21	50.11	37.79	15.75	14.29
C	3.85	3.74	99.98	50.17	49.86	37.29	15.80	14.21
D	3.74	3.65	100.30	49.95	50.00	37.50	15.06	13.57
E	3.01	3.81	100.06	50.07	50.15	32.44	15.61	14.23
F	3.05	3.86	100.12	50.05	50.14	37.55	15.53	14.10
G	3.87	3.76	100.15	50.08	50.25	37.42	15.37	13.93
H	3.63	3.52	100.28	49.93	50.15	37.17	14.42	13.02
I	3.90	3.81	99.88	50.05	49.95	37.54	15.58	14.60
J	3.99	3.90	100.00	49.93	50.11	37.54	16.08	14.41
Totals	38.51	37.52	1000.77	500.45	500.29	369.32	154.21	140.09
Binomial value	38.46	37.72	1000.00	500.00	500.00	375.00	153.85	141.01

A comparison of the values of E_S and V_S (the Stevens' expectation and variance) with the binomial values shows that the agreement is very close and the reason for this is apparent from Table II which exhibits the number of times each of the 52 target symbols occurs in the total of 2,000 trials and the number of times each symbol is guessed.

TABLE II

Targets	(a) SPADES												Totals
	1S	2S	3S	4S	5S	6S	7S	8S	9S	10S	JS	QS	KS
Targets	44	38	43	43	32	40	36	35	39	38	38	40	35
Guesses	36	17	29	28	52	10	21	45	40	71	31	38	50
Targets	(b) CLUBS												Totals
	1C	2C	3C	4C	5C	6C	7C	8C	9C	10C	JC	QC	KC
Targets	37	37	36	36	42	43	37	36	46	36	39	37	41
Guesses	38	9	50	41	77	24	27	29	35	63	41	47	38
Targets	(c) DIAMONDS												Totals
	1D	2D	3D	4D	5D	6D	7D	8D	9D	10D	JD	QD	KD
Targets	40	35	34	37	38	36	31	39	44	35	39	41	43
Guesses	47	24	44	35	62	17	29	29	41	53	30	24	56
Targets	(d) HEARTS												Totals
	1H	2H	3H	4H	5H	6H	7H	8H	9H	10H	JH	QH	KH
Targets	41	33	41	37	41	38	42	34	49	40	37	36	35
Guesses	65	14	35	33	71	12	23	31	70	52	21	33	53

For the whole 52 target values, assuming equal expectations for each symbol, we find $\chi^2 = 17$ with $n = 51$. For a binomial distribution we might expect χ^2 to lie between 50 and 72 and hence the

numbers of each symbol are far *more* equal than would occur in a binomial distribution.

The effect of this equalization is to cause the Stevens' expectations and variances to approximate closely to the binomial values. As might be expected with playing cards the number of guesses vary between wide extremes viz. from 9 on 2C to 77 on 5C.

The Cross Checks

A Stevens' distribution of targets is not of course random in the sense that a Bernoulli distribution is random since the numbers of the various symbols need not be even approximately equal and the chances of scoring a hit varies from one guess to another. In order, for instance, to randomize one of Dr Rhine's packs it would theoretically be necessary to have all the $\frac{25!}{(5!)^5}$ permutations of the 25 cards tabulated and select one from a certain page and column by means of an ordinary table of random digits. The 4-4 case has, I understand, been thus tabulated, but the 5-5 case contains too many permutations to make tabulation a practical proposition.

In our case the best way to discover how the distribution of targets behaves in practice is to carry out an extensive and systematic cross-check. This was planned as follows: There are 10 sittings with 4 sheets each and each sheet contains two columns of 25 trials. Thus we may label the 8 columns of sitting A in chronological order $A_1, A_2 \dots A_8$ and similarly we shall have $B_1 \dots B_8$ for the second sitting, and so on. By taking a pair of columns say A_2 and A_5 from the first sitting we can match the guess column of A_2 with the target column of A_5 and also the target column of A_2 with the guess column of A_5 . We thus obtain a score of hits on a total of 50 trials. In this way for sitting A we obtain 28 sets of 50 trials and 28 cross-check scores. In all we shall have 280 cross-check scores for the ten sittings A-J. By adding the scores on the cross-checks $A_{2,5}, B_{2,5}, \dots J_{2,5}$ we obtain a score on a total of 500 trials which we can denote by (2, 5). The 28 sets such as (2, 5) can then be arranged in the following scheme:

No. 1	(1, 2)	(3, 7)	(4, 5)	(6, 8)
No. 2	(1, 3)	(2, 4)	(5, 8)	(6, 7)
No. 3	(1, 4)	(2, 3)	(5, 6)	(7, 8)
No. 4	(1, 5)	(2, 7)	(3, 6)	(4, 8)
No. 5	(1, 6)	(2, 8)	(3, 4)	(5, 7)
No. 6	(1, 7)	(2, 5)	(3, 8)	(4, 6)
No. 7	(1, 8)	(2, 6)	(3, 5)	(4, 7)

This provides us with 7 cross-checks each of 2,000 trials. Thus in each of the cross-checks Nos. 1-7 each of the eight columns of every sitting is used once and once only. Further the seven cross-checks have no matchings in common.

Overall results

For the 14,000 trials of the seven cross-checks the observed number of hits on card, colour, suit and value together with the binomial expectations are given in Table III.

TABLE III

	<i>Card</i>	<i>Colour</i>	<i>Suit</i>	<i>Value</i>
<i>Observed</i>	266	6979	3447	1135
<i>Expected</i>	269.2	7000	3500	1077
<i>Deviation</i>	-3.2	-21	-53	+58
<i>Binomial S.D.</i>	16.2	59	51	31.5

It is seen that the deviations on card, colour and suit are nowhere near significance and that the deviation on value is less than two S.D. This last deviation cannot be given much weight since it is the most significant chosen out of four.

Overall Variance of the Cross-check

In the cross-check there are 280 sets of scores each made on 50 trials. The observed variances per 50 trials on card, colour, suit and value are estimated as follows:

(a) Card

Observed mean score per 50 trials = $266/280 = 0.9500$

Sum of squares of scores = 514

Hence variance = $280/279 (514/280 - (0.95)^2)$
 $= 0.9365$ per 50 trials.

This gives an observed variance for the 14,000 trials of 262.22 i.e. a S.D. = $\sqrt{262.22} = 16.2$ which agrees exactly with the theoretical binomial standard deviation.

(b) Colour

Observed mean score per 50 trials = $6979/280 = 24.925$

Sum of squares of scores = 177,779

Hence variance = $280/279 (634.9250 - 621.2560)$
 $= 1.0036 \times 13.669$
 $= 13.7182$ per 50 trials

And observed variance for 14,000 trials = $13.7182 \times 280 = 3841$

i.e. observed S.D. = $\sqrt{3841} = 61.98$ as compared with the binomial S.D. of 59.16

(c) *Suit*

Observed mean score for 50 trials = $3447/280 = 12.3107$

Sum of squares of scores = 44,695

Hence variance = $280/279 (159.625 - 151.553)$
= 8.100 per 50 trials

And observed variance for 14,000 trials = 2268 i.e. S.D.
= 47.623

as compared with the binomial S.D. = 50.235

(d) *Value*

Observed mean score for 50 trials = $1135/280 = 4.05357$

Sum of squares of scores = 5,547

Hence variance = $280/279 (19.8107 - 16.4314)$
= 3.3914 per 50 trials

And observed variance for 14,000 trials = 949.59 i.e. S.D.
= 30.7

as compared with the binomial S.D. = 31.5

If, however, we work out the variance for cross-check for Value, not about the observed mean score of 1135, but about the binomial expectation of 1077 we find this variance to be 1037.2 so that the standard deviation is 32.1 which is again in good agreement with the binomial estimate of 31.5. Hence the observed variances of the cross-checks of 14,000 trials are in good agreement with the binomial values and, as we have seen previously, these agree closely with those derived from Stevens' formula.

In Table IV(a) we give the scores for the seven individuals cross-

TABLE IV(a)
Cross-check scores

No.	Card	Colour	Suit	Value
1	37	975	477	177
2	39	1028	494	161
3	40	1015	507	162
4	43	976	487	159
5	38	1004	500	165
6	36	972	487	173
7	33	1009	495	138
Totals	266	6979	3447	1135

checks. The binomial expectation and variance for each cross-check of 2,000 trials appear in Table IV(b).

The only slightly abnormal result in the above table is the score of 177 for Value in cross-check No. 1, but even this is less than 2 S.D. and is chosen from 28 results.

TABLE IV(b)
Expectation and Variance for 2,000 trials

	<i>Card</i>	<i>Colour</i>	<i>Suit</i>	<i>Value</i>
<i>Expected</i>	38.46	1000	500	153.85
<i>Binomial S.D.</i>	6.14	22.36	19.36	11.92

It would of course have been possible to increase the number of cross-check matchings by using pairs of columns such as $A_2 B_5$ from different sittings but as guessing patterns might well vary from one sitting to another we thought it best to confine ourselves to all possible pairs selected from the same sitting.

Although the target sequence for each of the experiments A-J does not satisfy a Poisson distribution the number of hits on the card are consistent with such a distribution.

There are in all 40 sheets of 50 trials each. In Table V, I compare the number of sheets with 0, 1, 2, 3, ≥ 4 hits with the Poisson expectations. The average number of hits per sheet of 50 is $78/40 = 1.95$. Noticing that $\exp. (-1.95) = 0.1422$ we obtain Table V.

TABLE V

<i>No. of sheets with:</i>	<i>0 hits</i>	<i>1 hit</i>	<i>2 hits</i>	<i>3 hits</i>	<i>+ hits</i>	<i>Totals</i>
<i>Expected</i>	5.690	11.097	10.819	7.032	5.360	39.998
<i>Observed</i>	3	11	14	9	3	4.00

Whence $\chi^2 = 3.796$ with $5 - 2 = 3$ df and $0.3 > P > 0.2$ which is not abnormal.

Similarly we find that the numbers of target symbols on which 0, 1, 2, ≥ 3 hits were made are also distributed according to Poisson's law:

TABLE VI

	<i>Observed</i>	<i>Expected</i>
<i>No. of cards with 0 hits</i>	14	11.603
<i>1</i>	17	17.404
<i>2</i>	9	13.053
<i>≥ 3</i>	12	9.890
<i>Totals</i>	52	51.950

With $n=4-2=2$ df we have $\chi^2=2.212$ whence $0.5>P>0.3$ which is not abnormal.

II. DISCUSSION AND FURTHER DATA

Let me say first that I disagree fundamentally with Mr J. F. Nicol's cavalier disparagement of the cross-check.¹ When we find (as we did in Mrs Stewart's 5 symbol experiments) that not only was there consistent agreement in the cross-check between the actual number of hits and the binomial expectation but also between the observed and the binomial variance extending over a total of 33,500 trials and that at no point in the work did the critical ratio exceed 1.5, then we have every reason to believe that had there been no ESP our results would have been close to expectation in spite of the deficiency of ABA patterns. Actually we did fit the run-scores to a binomial distribution with $p=1/5$ and found that $0.1<P<0.2$. And because the seven cross-checks in the playing card experiments are in similar agreement both as regards expectation and variance we find no reason to abandon our original conclusions in this case also.

What the critic has to explain is the consistent *positive* trend of Mrs Stewart's scoring over a series of 37,000 trials. If she sometimes produced scores significantly above expectation with other batches that were significantly below it there might be some grounds for supposing that a repeated pattern in her guess-sequence got somehow into step with a similar pattern in the target series. But this kind of effect would show up in the cross-check and would indeed be very obvious on the original target sheets. I have failed to discover in the target series of numbers 1-5 any such persistent patterns which would explain the high scores.

It is hard to understand what Mr Nicol is driving at in his remarks. Does he suppose for instance that the consistent 'chance' scores produced in 'clairvoyance' experiments were due to some kind of statistical artifact? And does he suggest that when the rate of scoring was speeded up the consistent appearance of (-1) scores which as consistently returned to normal when the ordinary rate of calling was resumed is also the product of non-randomness? Or that the consistent difference in the scores when two agents were in opposition requires no explanation other than that there was a deficiency in ABA patterns? If he really believes these things I imagine few will agree with him.

It is ten years since we carried out the last experiments with

¹ *International Journal of Parapsychology*, Vol. 1, No. 1, p. 49.

Mrs Stewart and it seems very unlikely now that I shall discover how or why there was a shortage of these patterns. What I am certain is that 'non-randomness' cannot account for the various findings and that the conclusions described in our book *Modern Experiments in Telepathy* are all valid.

It is worth while however to indicate another line of approach. Instead of taking our targets and guesses in vertical columns of 25 trials let us arrange them in horizontal rows. This check I shall report only so far as I have carried it to date. Let us confine ourselves to the first or 'A' columns of the 670 'telepathy' sheets, omitting as usual the last ten sittings at which only 'chance' results were obtained. We thus consider 25 horizontal series of 670 trials taken in chronological order. In each row there is only one target number selected from a given sheet and the 'code' is changed at the end of the sheet. It might be assumed that if the numbers 1-5 are decoded into the five letters E, G, L, P, Z etc. there was a reasonable likelihood that a row of 670 letters would give an approximately random series considered apart from the other rows.

So far I have taken the first five rows of the 'A' columns of the 670 'telepathy' sheets mentioned in *Modern Experiments in Telepathy* (p. 321). These provide a total of 3,350 trials on which the scoring rate is about 7.66 hits per 25 trials and the critical ratio is about 15. As explained on pp. 383-4 the three sets of different target symbols E, G, L, P, Z; C, F, H, R, T; C, F, H, K, T; are represented in alphabetical order by A, B, C, D, E. The observed frequencies of A, B, C, D, E in the first five rows of the 'A' column of each sheet are as given below:

Row No.	A	B	C	D	E	Totals
1	140	128	142	124	136	670
2	120	133	129	136	152	670
3	152	120	121	143	134	670
4	135	137	129	143	126	670
5	127	142	121	128	152	670
Totals	674	660	642	674	700	3350

χ^2 tests applied to each row give:

$\chi^2 = 1.88; 4.10; 5.74; 1.34; 4.79$, respectively, each with 4 df. None of these is abnormal. The sum of the five values is 17.85 with 20 df. This gives $0.7 > P > 0.5$ which is again normal. It is

seen therefore that the changing of the code after each sheet has effectively equalized the frequencies of the five symbols.

Single or Isolated Targets

The expected number of these in 670 trials is 429.12 and the observed numbers are 426, 424, 419, 432, 432, none of which differs significantly from the expected number. The expected total of such 'isolated' targets on the five rows joined into a single series is 2145.6 as against the observed number of 2133 which is in good agreement.

Patterns ABA

The expected number of ABA patterns in an uninterrupted run of 670 trials is $4/25 \times 668 = 106.88$. The observed numbers for the five runs are: 110, 105, 109, 105, 112, giving a total of 541 compared with an expectation of 534.4. The total expected variance is 448.9 giving an S.D. of 21.2. Thus the number of ABA patterns approximates very closely the theoretical expectation.

So far then as these five rows are concerned they pass three tests for randomness. It is possible of course that there is a considerable vertical linkage between corresponding members of different horizontal series and a far slighter horizontal linkage between members of the same series. Because the series passes three tests it does not follow that it is random, but then the same may be said for any series. What is probably true is that the horizontal series are much more random than the vertical columns. In any case it is obvious enough that the high scores are not a direct result of the deficiency of ABA patterns. The shortage of such patterns could in the case of a subject like Mrs Stewart who scores high on the 'target' card have seriously affected our expectation of ± 2 hits but we did not discuss this problem.

Perhaps the most important properties of a target series are (a) that the cards are well-mixed and (b) that the numbers of the different symbols should be approximately equal. This last condition can be taken care of however if Stevens' formulae for expectation and variance are employed. Errors most frequently arise through the incorrect use of the binomial formulae in cases where a large excess of some particular target symbol happens to coincide with a similar excess of the same symbol in the subject's guesses.

No doubt it is true to say that careful randomization is essential in experiments with subjects or groups of subjects whose results show only an overall marginal significance with odds of the order of 100 or 200 to 1, more especially when such small odds are obtained

after many thousands of trials. Such results scarcely impress the average scientist and it might be better to institute an intensive search for high-scoring subjects, or, alternatively to devise a repeatable experiment. The latter consummation is probably at present unattainable.

But to return to the playing card experiments, I think the consistency of the various checks made in this paper indicate that some form of randomization was adopted though I was mistaken in saying that Fisher's tables were used. One method occurs to me which might have produced a similar distribution of targets could be to have mixed six complete packs of playing cards and to have drawn in succession 200 cards without replacement and with no sight of their faces. Unfortunately the problem of estimating the numbers of symbols which could occur 0, 1 . . . up to six times is far from easy and at least one university professor of mathematical statistics has so far been unable to solve it. I mention six packs since that is the number I possess and might have employed.

THE JONES BOYS AND THE ULTRASONIC WHISTLE

BY CHRISTOPHER SCOTT AND K. M. GOLDNEY

THE possibility that the Jones Boys¹ might have achieved their success in card guessing, on some or all occasions, by means of ultrasonic whistling, either with the tongue and teeth or lips, or with the aid of a mechanical whistle, was first brought to our attention in the summer of 1958 by Mr C. E. M. Hansel, a psychologist of Manchester University. Since that time we have taken a number of steps to investigate this hypothesis. While we do not consider that a definitive statement that they did or did not use an ultrasonic whistling technique can yet be made, we are anxious not to delay any further our report on our progress. The present article makes no attempt at an exhaustive analysis of the problem; its purpose is to put the main developments on record before they become ancient history and to make relevant facts available to anyone who may wish to assess the Jones Boys case.²

¹ See *The Mind Readers*, by S. G. Soal and H. T. Bowden. Faber, London, 1959.

² We use the term 'ultrasonic' somewhat loosely in this report to refer to very high pitched sounds that are inaudible to some people but not

On 2 August, 1958 C. S. visited the Jones family in Wales with Mr Hansel, Mrs Scott and Miss Anne Reynolds, a 13-year-old niece of Mr Hansel. Before the visit a few tests were carried out with an Edelman whistle brought by Mr Hansel. This is a variable pitch precision whistle, capable of emitting a very pure note up to a range well beyond that of any human ear. As far as we know it is not on sale in this country. It is quite an elaborate instrument, measures six inches in length and weighs half a pound. It is normally operated by a rubber bulb, about an inch in diameter, to which it is attached by a length of rubber tubing. It can be concealed in a large pocket or inside the trouser leg, but is bulky and heavy and could not reasonably hope to escape detection in a search. The ability to hear high frequency sound falls off fairly systematically with increasing age in the listener (though there are marked individual differences in sensitivity). We found it possible to adjust the pitch to a level at which it was inaudible to all three adults at a distance of 2 feet but readily audible to the child at much greater distances. Using this pitch, Miss Reynolds was coached for about half an hour before we called on the Jones family.

The experiments with the boys, which took place in the field behind the house, were almost devoid of interest. Ieuan, the agent, had been working and only one hour was available. In the event even this period was cut short by a sudden downpour, and only three runs were completed. Scores were in accordance with chance and no whistle was heard. Three observations were made which might have some relevance.

1. Before the experiments the boys were instructed to turn out their pockets by Mr Richard Jones (the father of Glyn, the percipient), who then pointed out that there was nothing suspicious in them, and in particular mentioned 'no whistle'. Dr Soal, who spoke to Mr Jones over the telephone from London just before this incident, has told us that he never mentioned, then or previously, any whistle hypothesis to any of the family. The question therefore arises why Mr Jones made this remark. Experience has shown us that this (like several other facts concerning the Jones Boys) is capable of being used equally by supporters and opponents of the ESP hypothesis. To abbreviate the arguments, the former can say: if they were really using a whistle surely they would not have risked this suggestive remark. The

necessarily to all. Strictly, ultrasonic sounds are inaudible to all, but such sounds obviously have no relevance to our discussion. The term 'supersonic' is sometimes wrongly used for 'ultrasonic'. 'Supersonic' means faster than sound.

latter: if they had never used a whistle how did Mr Jones get the idea? In our view both arguments can be readily answered and neither deserves much weight.

2. Throughout the three runs, when C. S. sat facing the agent Ieuan at a distance of 3 feet, he noticed the boy continuously moving his tongue behind his lips (or so it seemed). C. S. wondered whether this represented an attempt to make a whistling sound. Incidentally, at the end of the first run Ieuan opened his mouth wide in a yawn and C. S. was given a good view of its interior. No apparatus was visible. It should be added that Ieuan's complete immobility on most occasions during Dr Soal's experiments has been recorded by K. M. G.

3. All the observers, in listening for high pitched signals, noted a great amount of high frequency background noise, mainly, no doubt, from birds. Such noise normally passes unnoticed. Its presence, however, might make any systematic signal a good deal less conspicuous to someone who could in principle hear the signal.

At this stage the writers were somewhat half-hearted about the whistle theory. However, we made many (inconclusive) enquiries among sheep-dog experts to determine whether ultrasonic sound, used for controlling sheep-dogs, could be emitted, without accompanying audible sound, by the mouth unaided by apparatus. We obtained whistles which could be concealed in the mouth, but these were far from ultrasonic.

In March 1959 we ordered a Galton whistle from Messrs C. F. Palmer (London) Ltd, for the purpose of experiment. Hitherto we had been thinking in terms of an apparatus blown by the mouth (the bulky Edelman did not seem a practical proposition). When the Galton whistle arrived it turned out, to our surprise, to be operated by a small bulb which fitted over the end. It was smaller, lighter and cheaper than the Edelman (five inches long, including the small bulb of half an inch diameter; an ounce in weight and costing 4 guineas). Although the note was less pure, C. S. soon found that the accompanying hiss could be concealed by clothing, and after half an hour's practice he had worked up a simple trick with his wife which more or less duplicated the main Jones Boys experiments. He suspended the whistle under his shirt from a string around his neck, folded his arms and pressed the bulb through his coat and shirt with one finger, the movement being concealed by the arm which covered the hand. The frequency was adjusted to be audible to his wife and a simple code was agreed (one pip for E, two for G and so on). C. S. at once (21 March, 1959) invited K. M. G., Dr West and Mr Cutten to his house and

gave a demonstration. C. S. and his wife sat back to back, separated by 15 feet, in the same room. Scores around 20 out of 25 were achieved. K. M. G. (who alone knew the trick) heard and saw nothing, nor did Mr Cutten. Dr West, who watched C. S. very closely, detected the slight movement of the arm when the bulb was pressed, and found that then (though not before) he could just hear the whistle. We were able to demonstrate, however, that with a slight adjustment to C. S.'s arm position even this small movement could be eliminated. With a child as percipient, of course, the pitch could have been raised considerably and made quite inaudible to any normal adult.

Immediately after this demonstration we telephoned the late Mr Jack Salvin, the well-known magician and expert in trick mind-reading, who, it will be recalled, tested the Jones Boys on their third visit to London and certified that he was himself satisfied that no normal means of communication was used by them. Mr Salvin came over the same evening, and, following the Jones Boys procedure, we asked him to take complete charge and try to determine the nature of the trick. He failed completely. After three runs (during which he drew the curtains, asked Mrs Scott to take off her glasses, moved her chair one yard to the side, and for the rest simply watched) he expressed himself baffled—and immensely impressed. When we explained the trick he was unable to hear the whistle even when it was removed from under the clothing. (We found this to be generally true among those aged over fifty.) He told us, however, that long ago he had himself experimented with some kind of whistling device for magical effects.

The next day we put on a similar performance for Dr Soal, in his own house. He was equally mystified, and he also could not hear the whistle in any circumstances. A few days later we demonstrated for Mr Bowden. Here the conditions were more impressive. A blanket screen was erected between the agent and percipient. On one occasion Mr Bowden sat at C. S.'s left (the side on which C. S. had the whistle) with his right ear less than two feet from the whistle, and stared closely at C. S. On another occasion Mr Bowden and C. S. sat face to face, two feet apart, with no table between; during the run loud music was played on the radio. All scores were above 15 out of 25. Mr Bowden failed completely to detect any sound or movement and could offer no explanation. Our final demonstration was to Mr Alec Reeves, the acoustic expert whose discussion—and dismissal—of auditory signalling between the Jones Boys is reported in *The Mind Readers* (pp. 187-8). He too neither heard the whistle nor saw any movement.

These demonstrations led us to take the hypothesis about the use of a whistle by the Jones Boys very much more seriously, and our suspicions were further strengthened by the news from Dr Soal that a high pitched dog-whistle had been on sale in the Welsh village next to that in which the Jones family lived. We were inclined to argue as follows. Any trick performed by the Jones Boys would have to be such as to deceive Mr Salvin. Further, to be operated by the Jones Boys it ought to be something simple. There cannot be many tricks which would deceive Mr Salvin, and there must be even fewer that are simple. Nevertheless we had now found one that seemed to meet both conditions, and, having done so, we found a direct link between the method involved and the part of Wales where the boys lived. The coincidence seemed striking.

In discussion with Dr Soal and Mr Bowden it was agreed that a further test of the boys was essential under conditions in which any high pitched signal would be detected. Three methods of detection suggested themselves: the use of electronic equipment, the use of a human detector in the form of a young person with sensitive hearing, and a meticulous search of the agent immediately after a successful experiment.¹ The last we ruled out as both risky and inconclusive. The danger with the first two alternatives, however, was that if the boys intended to use a whistle their suspicions would be at once aroused by the presence of electronic equipment or of a young person. A further problem was that the boys had not scored successfully for two years (they had only once or twice been tested). Were they still capable of performing? Evidently a situation was required which offered them the highest motivation. All these requirements suggested one ideal solution, a television programme, which would offer the necessary incentive and, at the same time, allow electronic equipment capable of detecting ultrasonic sound to be present without arousing suspicion.

Contact was made with the B.B.C., who readily agreed to an appearance of the boys on the programme 'Panorama' on 27 April, 1959. We were not confident that any useful experiment could be performed on the programme itself and it was not our aim, nor that of the B.B.C., to make a public exposure of fraud; we hoped, however, that the programme would provide the boys with the strongest possible motivation to score successfully and that we would be able to detect any use of a whistle during practice runs and rehearsals. If they had succeeded, without any sound being

¹ The use of an animal detector was also considered but soon dismissed as impracticable.

detected, the case for the boys as genuine mind-readers would have been greatly enhanced. We invited the boys to London two days in advance of the programme. Unfortunately they arrived only the evening before and there was time for no more than one practice session that evening before the studio rehearsals the next day. During this practice session we arranged for a 12-year-old boy to be present who had been trained to listen for the whistle. No above-chance scores were obtained and no whistle was heard. The boys, however, appeared confident and in good spirits, and they told of a highly successful broadcast demonstration they had given a few weeks before on the Welsh Region radio. We were therefore moderately hopeful of some results in the studio.

These hopes were disappointed. Neither the rehearsals nor the broadcast produced any positive results. The B.B.C. arranged equipment to detect a whistle (and even to display the sound on an oscilloscope inset at the bottom of the broadcast picture) but without effect. The programme included a demonstration by C. S. and his wife of card-guessing using a whistle (score: 14 out of 15), and interviews by the broadcaster Richard Dimbleby with Dr Soal and Mr Hansel.

All experiments were conducted by Dr Soal and Mr Bowden. The boys' fathers were present. Persons known to be sceptical of the boys' ability, or to whom the boys were supposed to be antipathetic, were kept out of sight. The present authors and Mr Hansel were in the control room during the rehearsals and the broadcast, but their presence was not made known to the boys until after the boys' performance.

Interviewed by Richard Dimbleby on the programme after witnessing the whistle demonstration, the boys denied ever having used any such whistle. After the programme C. S. showed the boys the whistle and asked them if they had seen it or anything like it before. They said they had not. Their fathers gave the same answer.

We should add that all those concerned, including the B.B.C. team, agreed not to tell the Jones family before the programme of our suspicions regarding whistling, or of our precautions to detect it. (The family were not, however, wholly ignorant of the suggestion, since it is aired briefly in *The Mind Readers*, pp. 275 and 280.) While we would have liked to maintain this secrecy even longer, to allow further experiments with the boys, we knew that the matter was about to receive public discussion in *The New Scientist*,¹ so that this was to be our last chance of testing the

¹ 30 April, 1959, letter to the Editor from C. E. M. Hansel.

whistling hypothesis before it received publicity and possibly came to the attention of the boys.

We have made no further attempts to experiment with the boys.

One incident at the B.B.C. is well worth reporting. During the preliminary discussions with the two producers we were demonstrating the whistle when one of the producers remarked, 'I think I could do it without the whistle'. He then made a high-pitched very quiet whistling sound between his tongue and teeth, while holding his expression normal and completely relaxed. There was no visible movement. The sound was faint and might easily escape notice, though once detected it was readily audible 20 feet away. What surprised and interested us most, however, was that the other producer, a young man, was apparently unable to hear the sound from any position in the room. We propose to make further experiments with this means of communication.

We have carried out a number of informal tests with whistles among both adults and children. These have not been rigorously conducted and we hesitate to submit our conclusions for publication. However, in the apparent absence of any systematic published data on the subject it seems equally wrong to withhold our own results. We would advise any interested enquirer to carry out experiments of his own.

The most important general finding is the unreliability or instability of the instruments under the conditions in which they are likely to be used for trickery. With the whistle concealed under the clothing it is quite common to experience complete failure. This may be because the outlet of the whistle is liable to turn against the body, or to be blocked by a fibre from the clothing—we have not confirmed any explanation. While this unreliability may often lead to complete failure, lasting until an opportunity is found to readjust the instrument or its position, it does not lead to frequent *spasmodic* failures. Thus, once the whistle is working it usually continues to work throughout at least a run. The occasional signal may be missed, but a score of about 20 out of 25 can usually be relied upon. With the Jones Boys, however, such very high scores were exceptional. To explain scores at a lower level in terms of whistling, one must assume either (a) deliberate restraint, or (b) the use of a code which differentiated only certain groups of cards (e.g. the red from the blue) and not each individual symbol, or (c) a less reliable instrument or one that is more tricky to operate.

Besides unreliability in the whistle we find instability in hearing it, depending on small changes in the conditions. Thus on some occasions we failed through a closed door while on others we

succeeded. This may have depended on the room or the door, but also much depends on the position of the sender and receiver within the room. In certain positions, even in the same room, the sound sometimes almost vanishes.¹

We have already mentioned how sensitivity varies between individuals, and markedly with age. It also improves considerably with practice, and it depends a great deal on knowing what to listen for. It is surprisingly easy for a person not to hear the sound when he is not expecting it even though it is well within his range of audibility. This is an important point to remember when attempting to assess whether any particular observer of the Jones Boys experiments would have heard a whistle had one been in use. One interesting psychological observation is the extreme surprise shown by an observer who can hear the whistle when he finds that someone else cannot. Apparently people find it difficult to accept the existence of wide individual differences in sensory thresholds.

With the whistle concealed under the clothing the maximum distance at which we have succeeded in sending an audible signal to a child is 130 feet. Under such conditions it was inaudible to a middle-aged adult two or three feet away. With the whistle outside the clothing a distance of 180 feet was reached, but the whistle will then of course be visible (unless, perhaps, hidden beneath a table or chair), besides emitting a hiss which is audible to anyone within a few yards. The Jones Boys were reported successful on three occasions at distances above 130 feet, their greatest distance being 166 feet. To explain this in terms of a whistle one might assume (*a*) that their whistle was slightly louder, or (*b*) that the clothing concerned was less absorbent to the sound, or (*c*) that the percipient was more sensitive than any of the five children we tested, or (*d*) that someone other than the agent operated the whistle, or (*e*) that someone other than the percipient heard the signal and relayed the information on to the percipient.

We tested the whistle in a strong wind with light rain. We were unable to find any difference in carrying power dependent on the direction of the wind. We have not found any explanation of this rather surprising result.

On one occasion when testing children indoors we found the whistle audible from another room when the whistle and the

¹ This may well be a standing wave phenomenon, due to interference between the outgoing wave and a wave reflected from the wall or ceiling. The effect can be readily observed by walking about a room in which a high audio-frequency oscillator is in continuous operation.

listener were in line with an open door between them. Movement of the whistle a few feet out of line made it inaudible. This closely parallels one of the observations with the Jones Boys. It is well known that high pitched sound is more directional than low pitched sound. This means that it is less able to bend round corners, not that it is *emitted* in one direction. In practice it can nevertheless often go round corners if the whistle is operated indoors, owing to reflection by walls, etc.

For most of our experiments we used the Galton whistle, and for a few the Acme 'silent dog whistle'. The latter is a sheep-dog whistle, three inches long,¹ weighing three quarters of an ounce and costing 11/- . It is not sold with a bulb, but one can of course be attached. The highest pitch normally obtainable is (despite the name) readily audible to reasonably healthy adult ears, but by unscrewing a small lug screw and removing a spring (an extremely simple operation which could happen almost accidentally in the course of idle fiddling) one can raise its pitch into the range at which it is inaudible to most adults. This whistle is a relatively crude instrument and requires a rather sharp puff of air for its operation. We had considerable difficulty in obtaining a bulb which would deliver the necessary puff without itself becoming audible. However, if it were desired to obtain only a modest score, a single-puff code could be used which, though in theory audible, might well escape notice.

We have deliberately said nothing in this article about the actual frequencies emitted by the different whistles and audible to the people we have tested. We have not calibrated any of the whistles ourselves and, although we have some idea of the frequencies concerned, it seems to us preferable to avoid any numerical statements until rigorous experiments have been carried out. We have, however, made an extensive search of the literature on auditory thresholds, but have found very little information on the absolute high frequency threshold and its relation to age. The standard work in this field is that of Bunch² (indeed this seems to be the *only* work on this topic which covers frequencies above 10 kc.), but Bunch's work was limited to adults. The only comparable work on children seems to be that of Ciocco,³ but he used no frequency above 8,192 c., which is easily audible to all normal children and most adults and therefore of little interest to us. Thus, somewhat surprisingly, there seems to be no evidence on record regarding the high frequency auditory thresholds of children at different

¹ The operative part is much shorter. Mr Hansel has shown us one which he has cut down to 1 in. in length without affecting its performance.

² Bunch, C. C., 1929. *Arch. Otolaryngo*, 9, 625-36.

³ Ciocco, A., 1936. *U.S. Dept. Health: Publ. Health Rep.*, 51, 1609-21.

ages. We are, however, continuing to search the literature and would appreciate any leads from readers of this journal.

Bunch's report on adults (*loc. cit.*) is somewhat inaccessible. Unfortunately it is not easy to summarize since four distinct factors are of interest: frequency of the tone, intensity of the tone, age of the subject, and extent of individual differences between subjects. The results may be summarized roughly in two statements:

1. There is a marked drop in the frequency of the highest audible note with increasing age (1.5-2 kc. per 10 years of age would be a rough approximation).
2. Superimposed on this there are wide differences between individuals.

The following is a typical set of results showing how many subjects heard the 16,384 c. tone at the peak intensity used:

<i>Age range</i>	<i>Number tested</i>	<i>Number hearing 16kc.</i>
20-29	68	68
30-39	70	38
40-49	78	9
50-59	85	1
60+	52	—

With children, the most that we can say is that, in our experience, the improvement of hearing continues down the age range to pre-adolescence at least. We are under the impression that young children can hear 25 kc.

It has been suggested to us that a thorough and scientific enquiry into the properties of ultrasonic whistles would settle the Jones Boys question one way or the other. For our part we do not share this view, although we would welcome such an enquiry. There are so many unknown factors in the Jones case that we do not see any possibility of drawing firm conclusions about the boys' performance from a study of whistles. For example, supposing they used a whistle, we do not know what type of whistle they used, how hard they blew it, how much the sound was damped by their clothing or how sensitive was the percipient's hearing. It is impossible to lay down limits for the performance of a whistle unless these factors are known. The most that can be done is to note some of the typical features of communication with whistles and compare them with general features of the Jones Boys' performance.

Our concentration, in this article, on one particular means of trick communication may have produced a false emphasis. We hope that nothing we have said might lead the reader to suppose that there are precisely two alternatives to be considered: that

the Jones Boys always used an ultrasonic whistle, or that all their successful scores were due to extrasensory perception.

Finally, we would stress that there are many arguments both in favour of and against the theory that the Jones Boys were using an ultrasonic whistle. We have avoided, as far as possible, discussing these arguments in the present article, whose sole purpose has been to describe the various investigations we have made and to indicate their relevance. Discussion of the pros and cons is better left to a separate occasion.

Implications

In our opinion there are a number of issues regarding ultrasonic whistles which would be worth careful thought by all who are interested in psychical research.

1. Why has no one mentioned ultrasonic whistling before as a possible means of counterfeiting telepathy? We know of no reference in the literature to this possibility. Such whistles have been known for a very long time. Psychical researchers are supposed to be familiar with all methods of trickery. Why did none of us think of this method?

2. Why did Mr Salvin fail to detect the whistle trick when performed by C. S. and his wife? Does his failure mean that conjurors, though skilled in the *performance* of tricks, are no better than the rest of us in the detection of unfamiliar methods? Could it be that he *did* detect the trick but was constrained to deny it by his professional ethic as a magician?

3. Mr Reeves, a distinguished expert in acoustics, witnessed the first two Salvin sessions in which the boys scored successfully at a distance of 27 feet. On page 187 of *The Mind Readers* he writes:

I was satisfied beyond any reasonable doubt that no auditory or visual signals took place; if they did both boys must be quite exceptional freaks of nature, having the ability to communicate acoustically with each other by some mechanisms and in some frequency range unknown to present-day anatomy.

Whether or not the boys were actually whistling, we now know that any normal boys *would* be capable of communicating with a whistle, inaudible to an adult, in just the conditions witnessed by Mr Reeves. It is no disrespect to Mr Reeves to suggest that he has assisted in the re-demonstration of an ancient truth: that the expert may well be wrong. We believe that this general problem would be well worth discussion by psychical researchers. Are there circumstances in which the expert *can* be relied upon?

Are there certain situations or types of problem in which he is particularly likely to go astray? Can we rely on the expert only when he says something is *possible*, never when he says it is *impossible*?

4. If a new pair of apparently successful 'mind readers' present themselves, what experimental precautions would be necessary next time to rule out ultrasonic signalling?

5. If so many of us overlooked a possible means of trick communication for so long, may there be other means which even now have not occurred to any of us?

The above reflections in no way depend on the assumption that the boys used trickery. We believe that they merit discussion whatever the final verdict in the case of the Jones Boys.

REVIEWS

WATER WITCHING U.S.A. By Evon Z. Vogt and Ray Hyman.
University of Chicago Press, 1959. 248 pp.

Water witching is better known here as water divining or dowsing. Vogt, an anthropologist, and Hyman, a psychologist, begin their book by stating the paradox which dowsing presents. They describe how a typical American family, the Bonds, faced with no cash, two dry wells, a thirsty dog, and other immediate water needs, finally turned to a diviner who successfully located a well for them. The experience converted the Bonds from sceptics to vociferous believers. Vogt and Hyman comment:

The conversion of the Bonds contains all the ingredients that led us to study water witching. The Bonds are well-educated and sensible people. They know that water witching is considered unscientific. Yet, when the chips are down and they have a lot at stake, they go against what their education and their reason tell them. Under conditions of extreme uncertainty and anxiety, they turn to the water witch. It is this aspect of water witching that caught our attention. Here is an apparent paradox. Here are people who ordinarily guide their lives by the latest scientific knowledge but who forsake science in their hour of need and resort to pseudo science.

It would be a mistake to conclude from this tendentious paragraph that Vogt and Hyman set out with preformed beliefs to debunk water divining. That was not their goal. They are more concerned with dowsing as a social phenomenon. 'We are interested in how people cope with the environment under conditions of uncertainty and anxiety.' It would have been better if

the authors had refrained from employing the term 'pseudo science' until the final chapter; for their survey of water divining in all its aspects has been both fair and thorough.

Vogt and Hyman set out to answer the basic questions 'Does the divining rod in fact find water?' and 'What makes the rod move?' and also a number of allied questions on the prevalence of dowsing in U.S.A., the conditions—geographical, geological, meteorological, and cultural—in which it occurs, and something about what makes American diviners tick.

They started by despatching a questionnaire (reproduced in full in Appendix I) to several thousand country agricultural advisory officers. The surprisingly high response rate of 72% was achieved, and the forms were filled in, for the most part, conscientiously. They gathered further information by interviewing and observing dowsers in action in five States and by way of a voluminous correspondence. The data were analysed by punched card techniques.

The question whether water divining works, the authors point out, is not a simple one. The 'facts' amassed by supporters of dowsing are of two kinds. The first kind is anecdotal. The second kind comprises 'field experiments, laboratory experiments, and tests of consistency.' Chapter 3 deals with the anecdotal type of evidence which comes from case histories and in which no objective standard is available against which one can evaluate the dowser's performance. Vogt and Hyman devote some space to a discussion of the fallibility of human testimony, and to the difficulty of assessing the results of field tests which are not backed by adequate controls. For example, Sir William Barrett concluded a report of his testing of William Stone: 'We have thus an experiment which conclusively proves the reality of dowsing', while the geologist, Gregory, summarizes the same test as follows: 'but the line he chose was that which I think any person experienced in finding water would have selected from obvious surface indications'.

The authors state how they would plan a controlled field experiment and give praise to the 1949 experiment conducted by Dale *et al.* for the American Society for Psychical Research, in which twenty-seven diviners were matched on equal terms against two 'experts', a geologist and a water engineer.

The results showed that the experts did a good job of estimating the overall depth of water as well as the depth at specific points. Neither expert did a good job in guessing the amount of water to be found at specific points, although the engineer made a close guess on the overall estimate of the rate of flow. The diviners, on the other hand, were

complete failures in terms of estimating the depth or the amount of water to be found at their selected spots.

An experiment by Ongley in New Zealand is next described. Fifty-eight water diviners were tested in a series of five different controlled tasks, such as requiring the dowser first to locate an underground stream and then to return to it with his eyes closed, and asking two or more dowsers to check one another on the location of underground water. 'Not a single one of the water witchers made a record significantly better than chance.' Seventeen other diviners were tested on their claims relating to detection of metals, medical diagnosis, tracking of people, discovering the owners of lost objects, and detection of pressure of electrical fields. The results again were no better than chance. As Vogt and Hyman point out, the inability of the diviner to succeed in a controlled test situation suggests to the sceptic that water divining has no basis in fact; to the believer, however, the unsatisfactory results are clearly due to the inadequacies of the scientific approach. The diviner succeeds 'when it counts', unhindered by the artificialities of scientific control.

The authors now propound strong views:

It is a commonplace to say that the scientist dismisses water witching out of prejudice. While it is probably true that many scientists would be prejudiced against such a proposition as water witching, the argument we are presenting is not one based on prejudice... we don't have to resort to prejudice to dismiss water witching as invalid. The evidence for it, when assembled and examined, is not merely insufficient according to current scientific standards (the same ones we would apply to 'acceptable' and plausible hypotheses), it is appallingly negative. We know few other hypotheses that have been put forth with such consistently negative experimental findings as the hypothesis that water witching 'works'.

If experiment refuses to support them the dowsers resort to other weapons and Vogt and Hyman deal with some of their arguments under such headings as 'the one-good-case argument', 'the test-of-time argument', 'the unfavourable atmosphere argument', 'the they-persecuted-Galileo argument' and so on. As an illustration of the passionate way in which people cling to a conviction, Vogt and Hyman narrate an anecdote. It is not greatly relevant but it is so good as to provide an excuse for quoting it.

The patient who believes that he is dead, is brought before the psychiatrist. The psychiatrist employs his best logic and psychological persuasiveness to convince the patient that he is still alive. But to no avail. Suddenly the psychiatrist has a flash of insight:

PSYCHIATRIST: Tell me, do dead men bleed?

PATIENT, (after carefully considering the question): No, dead men do not bleed.

PSYCHIATRIST (takes a pin and pricks patient's finger, whereupon blood gushes forth): Well, now what have you to say for yourself?

PATIENT (contemplating the bleeding digit): Well, by golly! I guess dead men do bleed after all!

The middle section of the book tackles the question 'Why does the rod move?' and the authors conclude that the movement is always the resultant of mechanical forces applied or removed by muscular action of the diviner. As a preamble Vogt and Hyman review at some length the various talking animals, the pendulum, the Ouija board, table turning, automatic writing, and hypnotic phenomena. In particular, $4\frac{1}{2}$ pages are devoted to the researches of Michael Faraday, the great physicist, into table turning—experiments that are probably not as well known as they should be to members of the S.P.R. They are described in a letter to the *Athenaeum* (the precursor of the *New Statesman*) of 2 July, 1853, pp. 801-3.

First Faraday made sure that the table turning would occur with the chosen subjects under his laboratory conditions, thus disposing of the 'unfavourable atmosphere' hypothesis. Further he established that the phenomenon occurred in the presence of the mechanical devices he fixed to the table. One of these was a pointer which gave an instant visual indication of any lateral pressure by the hands of the sitters. Anybody who has attended a Ouija board session or tested a dowser or pendulist will agree with Vogt and Hyman that 'the most characteristic feature of the phenomena we are describing in this chapter is the subject's vehement insistence that he is not the agency through which they are occurring.' When the rod or table moves 'the subject has no feedback from his hands and muscles to tell him that he is the agency. . . . It was Faraday who saw the necessity of giving the subjects this necessary feedback'. The authors quote passages from Faraday's letter, among them the following:

As soon as the index is placed before the most earnest, and they perceive . . . that it tells truly whether they are pressing downwards or only obliquely; then all effects of table-turning cease, even though the parties persevere, earnestly desiring motion, till they become weary and worn out. No prompting or checking of the hands is needed—the power is gone; and this only because the parties are made conscious of what they are really doing mechanically, and so are unable unwittingly to deceive themselves.

(Such thoughts, incidentally, were uppermost in my mind when

I visited Mr George de la Warr at Oxford. No insistence could have been more vehement than that of Mrs and Miss de la Warr that they were exerting no increased pressure on their rubber membrane at the moment when a 'stick' was obtained. If Mr de la Warr would get his workshop people to make him a Faraday indicator, he too would find that he and his wife and daughter are unwittingly deceiving themselves.)

The authors go on to describe research work that has been done by psychologists on unconscious muscular action and on the effect of suggestion. Curiously no investigation of this kind seems to have been devoted specifically to the *modus operandi* of the divining rod.

An early chapter of the book is devoted to the history of the subject and the final section is devoted to examining what sort of people take up dowsing and why and under what conditions dowsing flourishes. Vogt and Hyman conclude that 'water witching is a clear-cut case of *magical divination* in our culture which persists because there are potent psychological and social reasons for it', and in explaining their conclusion they have interesting things to say. They point out that there are many situations in which we must act, but cannot act rationally even if we want to; for example, when we have insufficient information by which to decide whether our actions are based on scientific criteria. This state of affairs often prevails when a farmer urgently needs water. The decision to drill a well is an important and costly one, and we should not be too hasty, say Vogt and Hyman, in ridiculing someone for resorting to dowsers. Pressure of time is a factor favouring the dowser. 'Contrast the clear-cut indications of the rod with the vague suggestions of the scientific geologist. The rod's message is decisive and unambiguous. The diviner says "dig here" and pinpoints the site precisely. The geologist supplies general information but leaves the pinpointing to the consumer'.

After some remarks on water witching as a cultural pattern, the authors reiterate that they regard it as a form of magical divination. 'We do not feel, however, that it is "superstitious nonsense" that should be stamped out at all costs, because, like other magical practices in our own and other societies, it does something important for people in uncertain situations that are, as yet, beyond the control of science.'

What effect would better education have on the persistence of water divining and other magical practices? Vogt and Hyman do not give the answer but express regret that so few college courses provide for any kind of training in scientific method.

With that criticism we, as members of a scientific body so closely concerned with the manifestations of pseudo science, must surely wholeheartedly agree.

DENYS PARSONS

THE ENIGMA OF SURVIVAL. By Hornell Hart. Rider & Co. London, 1959. 286 pp. 21s.

There is said to be now a declining interest amongst psychical researchers in the problem of survival. If this is so, it may well be because there is little new evidence on the subject. People have continued to get in touch through mediums with what appear to be surviving personalities, and have obtained more or less conviction of their identity. But so also had they when Myers wrote his *Human Personality*. The evidence of later investigators such as Drayton Thomas is more recent than that reported in *Human Personality* but not in any very important respect different from it.

The development of the Cross-correspondences was certainly a new line of evidence when it occurred but that is now some time ago. Most of the other attempts to develop new methods of research on this problem have led to results so unpromising as not to encourage other workers to pursue further research along the same lines. This is the case, for example, with Whately Carington's ingenious idea of applying quantitative psychological tests to communicators through different mediums. Equally discouraging are the results of the 'sealed package' test as carried out by Myers and Lodge, although I have some hope that a modification of that test may lead to more satisfactory results.

If this is the present situation, it does not mean that the survival problem must go, as a research problem, permanently into cold storage. It means rather that psychical researchers must think of new lines of attack that show some promise of being fruitful. Professor Hornell Hart has opened a new form of attack on the survival problem by re-exploring the question of apparitions of the dead. Since the publication in 1942 of Tyrrell's Myers Memorial Lecture on *Apparitions*, psychical researchers have been inclined to take it for granted that evidence for survival could not be drawn from this source. Professor Hornell Hart has, however, in his study of 'Six Theories about Apparitions' (1956), re-examined the evidence and argued not only that there were sufficient cases of high evidential value to establish the reality of apparitional phenomena but also that some apparitions of the dead show no significant difference from apparitions of the living. He has further argued that some at least of the latter must be regarded as vehicles through which a conscious personality

observes and acts, and has concluded that some apparitions of the dead are also vehicles of surviving human personalities.

The present book is not primarily a development of the author's views on apparitions, but rather a study of the whole survival problem of which his theory of apparitions forms a part. Its form is that of a presentation of arguments on each side of all parts of the problem. An opponent of the idea of survival might well complain that his side of the case is presented less forcibly than the other, and might feel that the book is rather a presentation of the case for survival than a balanced presentation of the cases for and against.

Professor Hornell Hart is concerned with the explanation as well as the establishment of the fact of survival. In Chapter 13 he develops a 'persona' theory of the communicators in mediumistic seances which he considers gets over many of the difficulties in other theories. The essence of this 'persona' theory is that the ostensible communicator is a personality structure developed with the aid of the persons present at the seance. This may be fictitious in various degrees but also it may be largely the product of a surviving spirit and be a vehicle for the activity and consciousness of that surviving spirit. This theory may also be used to account for apparitions.

This book shows wide knowledge of the literature of this subject and original thought about it. Even those who may find themselves in disagreement with some of its conclusions must recognise its qualities as a contribution to the discussion of the problem of survival.

R. H. THOULESS

CORRESPONDENCE

SIR,—Dr Figar's paper (*Journal*, 40, pp. 162–172) is extremely interesting, and may be very important. A number of points suggest themselves.

1. Was any mechanical check made on the independence of the two plethysmographs?

2. The agent's task consists of three parts: (*a*) he is called to attention (by the presentation of a card), (*b*) he presses a button, (*c*) he engages in mental calculation. Are we to take it that only the last of these has any effect on the plethysmogram? I should like to see the results of some dummy runs in which (*b*) or (*c*), or both, were absent; (*a*), of course, must always be present. This question does not affect the significance of the experiment, but is of some general interest.

3. In the experiment of Fig. 7, was the agent connected to the

apparatus? If not, this eliminates all question of instrumental interaction.

4. Since the percipient is not told about the experiment, we have no conscious orientation. We must therefore suppose that the experimenter's intention is sufficient to produce linkage. The alternative (which seems fantastic) is that the agent's bouts of calculation cause vascular reactions in *all* his friends and relations! In this connection it may be noted that in the spontaneous fluctuations recorded in Fig. 8, the roles of agent and percipient are apparently reversed, since the 'percipient's' reaction occurs first.

5. The coincidence of fluctuations during rest periods should be considered primarily as an undesired effect. As it is not known what causes these fluctuations, they may be due to independent responses to a common cause: e.g. subliminally perceived sounds, scents, temperature changes, etc. To eliminate these, it would be necessary (in spite of technical difficulties) to separate the subjects by a greater distance, to enclose them in heat-insulated rooms, etc.

6. The principal line of research seems to be indicated by Fig. 7. That is to say, the agent is not connected to the apparatus, and may therefore be at any desired distance from the percipient. He is given short bouts of mental calculation; the duration of these, and the interval between them, are recorded, and the percipient's plethysmogram is examined for a corresponding pattern. This is similar to the situation in radar work, where pulses are sent out and returning echoes are searched for. The radio-astronomers have developed a very sensitive method of detection, which was used recently for identifying radar echoes from the planet Venus. The received signals, which appear to the naked eye to consist entirely of random fluctuations, are analysed by a computer which examines them for correspondence with the pattern of pulses sent out. In this way even very feeble echoes can be detected. A similar technique applied to Dr Figar's plethysmograms would produce an extremely powerful method of research.

G. F. DALTON

Dr Figar comments on Mr Dalton's letter:

1. During the experiments there were always long phases which showed the curves recorded were independent of each other in respect, for example, of slow vasomotor changes and quicker pulse waves, the shapes, amplitudes and frequencies of which differed. Even in cases where apparently simultaneous vasomotor reactions are registered in the curves they are not actually synchronized and differ in detail. This shows that mechanically the two curves were not interdependent. In addition it was seen that

during the initial adjustments of the apparatus, such as the insertion of the hands into the plethysmograph chambers; the filling of them with water; the adjustment of the recording device, etc. there were no signs of interdependence although considerable fluctuations in the recording systems took place during these manoeuvres.

2. The question is one of the biological intensity of the stimuli to which the nervous system of the agent is exposed and, together with the other factors, requires thorough and careful study.

3. In the experiment represented by Fig. 7 the agent was not connected to the apparatus. It was one of the tests performed to observe the effect of increased distance between agent and percipient. In this case (Fig. 7) the agent was placed in an acoustically and electrically screened chamber about five metres from the percipient. In similar experiments of this type it was observed that the curve fluctuations were less constant and significant than when agent and percipient were in closer spatial contact.

4. Yes. Sometimes the fluctuations observed make it difficult to determine who is the agent and who is the percipient.

In general I agree with the opinions expressed by Mr Dalton.

SIR,—We ought not to resist whatever it is that a scientific experiment has to teach us; although if it turns out to be the opposite of what we expect it to teach us, the consequences may be hard to follow through. Might we not make a serious attempt to evaluate Dr Figar's results in this sense?

Accepting Dr Figar's becoming modesty about the inconclusiveness of his experiments we nevertheless have the suggested appearance of an ESP effect occurring at $2\frac{1}{2}$ times the expected rate while the 'supposed agent' is 'transmitting' mental effort, and at 5 times the expected rate while he is resting. Dr West was quite right to point this out as a 'surprising conclusion'.

Since the advent of quantum mechanics with its demonstration that to observe a particle is to *alter* it, scientists have become increasingly aware that this law may have much greater generality. The act of trying to test a phenomenon may indeed change its character. Let us assume that ESP exists. We know nothing of its mechanism, so the simplest hypothesis is that ESP is a faculty given to all men: a delicate faculty whereby some kind of harmony may be established between two people. To test this, we have imposed our own messages on the system, in order to see whether they can 'get through'. Loud, dramatic signals are hurtled into the system: stylized animals, playing cards, geometric devices. Perhaps these burst like bombs in the ordinary person's ESP

mechanism and utterly disrupt it. Only the lucky few, like Shackleton and Mrs Stewart, are able to adapt themselves to the 'noise level'.

Looked at in this way, the symbols everyone has been trying to transmit are analogous to the high energy photons with which the physicists tried to illuminate fundamental particles. Colliding with the particle, the photon disrupts its momentum in an unknown way—and further statements become impossible. Now apply this outlook to Dr Figar's results. ESP connections spring up between two 'resting' subjects. As soon as one engages in specific mental effort, the effect is halved. It would be most interesting to see whether the attempt to 'send' a specific symbol would destroy the plethysmographic correlation altogether. My hypothesis would suggest that usually it would.

Does not this outlook, suggested by Dr Figar's most interesting work, give a new impetus to ESP investigation? With the greatest deference to the pioneers, we have had decades of work based on the idea that ESP implies the transmission of a message; and we have reached 1960 with a mass of vaguely suspect evidence, a few sensitive percipients, and a great deal of argument about statistical artefacts. All this suggests we are looking for the wrong thing, and testing for it by dubious methods. The alternative hypothesis is that ESP establishes a kind of harmony, not normally adapted to transmitting messages, but possibly capable of adjusting a percipient's *own* thoughts into similar patterns to those of the agent. This would be a suitable mechanism to account for generalized premonitions and correspondences of mood which have no specific *content* as has a message, and which I suspect are far more common than the correspondence of factual information in the ordinary sense. We have tended to reject such evidence, because it is so subjective in its interpretation; but in insisting on objectivity we may have set up conditions in which ESP cannot normally operate. We might recall that 'objectivity' as the major criterion of a scientific result is a nineteenth century concept. The physicists were forced to abandon it years ago.

I suggest that we might now proceed like this. With all respect to the plethysmograph, vasoconstrictor reactions are no more than side effects of thought processes. The machine with which we think is the brain. Surely the instrument with which to experiment further is the electroencephalograph. We put two subjects in separate rooms, and leave them to rest: alpha rhythms are established by both brains and recorded. We then stimulate both subjects in a generally pleasurable way—perhaps by giving

them (different) picture books to look at. And so on. The hypothesis is that we shall find some kind of generalized activity, measurable perhaps in the dimension of pleasure and pain (I am thinking here of mood and premonition again) that will clearly demonstrate a correlation between the two encephalograms. And I should expect that, if such a harmony could be established, any attempt to transmit a message of factual content would then destroy that correlation.

STAFFORD BEER

SIR,—I was much interested in the article 'An Experiment in Apparitional Observation and Findings', appearing in your September issue.

I am not aware that any similar experiments have ever been made, and I cannot help wondering why not. The S.P.R. is much concerned with reports by persons who say they have seen apparitions, and the question raised is always the same: how strong is the evidence? One would have thought it would be of value in trying to answer this question, to have some knowledge of the way people generally react at the sight of an apparition. I hope, therefore, that the reported experiment will not be the last of its kind. I would, however, venture to make two suggestions in this connexion.

In the first place, I would suggest that the experiment did not go on long enough. It is true that 82 people passed, but a large proportion of these may have been looking in another direction (e.g. on the ground). I agree, however, that it is 'difficult to hold' that no one saw the apparition. My guess would be that those who saw it couldn't make out what it was, but assumed that there must be some normal explanation. These considerations suggest that the proportion of passers-by who both see it and go on to consider what it might be is very small. Hence the necessity for a large number of passers-by, if results are to be obtained.

Secondly, I would suggest that a number of people connected with the experiment should each go for a walk with a friend who knows nothing about it, and include one of the paths in the walk. In this way, it would be possible, so to speak, to study the reaction of an individual at first hand and to vary the conditions. For instance, the observer could say nothing, and just watch his companion; or, if the companion didn't notice the ghost, he could say to him 'What's that thing over there?', and see what happened.

C. W. MACMILLAN

SIR,—I know that members are interested in the cases of apparent spontaneous ESP which Freud, Jung, Ehrenwald, Servadio and other analysts have observed in the course of their work. Three reports of cases of this type are referred to in a recent article by Dr C. A. Meier, of Zurich, of which he has kindly sent me a copy.¹ These cases were first reported by an American child psychologist, Frances Wickes, in a paper she contributed to a collection made in honour of Dr C. G. Jung's eightieth birthday, and as this is not easily available I have made a summary of her third case which is of interest to psychical researchers.² It concerns the projection upon a son of his mother's excessive fears, of which they were both unconscious, and Dr Meier comments that 'it satisfies the most exacting criteria regarding the inconceivability of a mechanism of transmission.' In other words, there is an apparent indication of an extrasensory link between mother and son.

From childhood the mother had been exaggeratedly unselfish, living only for others as she felt that her own life was of no interest to her. She married an unstable young man who had been forced by his father into a career he hated. The marriage broke up and the mother then dedicated herself to her infant son, whom she adored. But her attitude to him was ambivalent. She was convinced that his father had failed in life, on the one hand because he had been forced out of his natural bent, on the other because of his own inherent weaknesses. The child resembled her husband and she was desperately afraid that he would follow in his footsteps. Consequently she felt that although he must choose for himself he must also be saved from himself. She rationalised this by deciding that he should choose for himself when he reached an age of discretion, but until then he must have only the opportunities chosen by her in an environment limited by her. Naturally enough in the end the boy took the only means of escape. He ran away.

His mother was in despair. She had no sense of a life of her own and would have committed suicide, had she not felt that he might come back and need her. Finally she turned to analysis, and Dr Wickes reports that after a long period and towards the end of an hour of treatment, there came to her 'a consciousness

¹ C. A. Meier, 'Projection, Transference and the Subject-Object Relation in Psychology.' Reprint from *The Journal of Analytical Psychology* 4, No. 1, 1959.

² F. Wickes, 'Three Illustrations of the Power of the Projected Image', *Studien zur Analytischen Psychologie* C. J. Jungs, I. (Rascher Verlag, Zurich, 1955).

of her own separate life . . . an individual potential which was hers to live, whatever might happen in the life of another'. She said, 'I am going to take up my own life and live it, even if I never see him again. My sorrow will go with me always but it will be my own life that I shall live'.

At that moment, by a conscious act of choice, she broke the link binding her to her son. A minute or two later the clock struck twelve and her day's treatment came to an end. Three days later she got a letter, dated the day of her decision, from her son, who had broken off all contact with her and thus did not know that she had been having analytic treatment. He wrote: 'Dear Mother, I am sitting on a hillside three thousand miles away. Just now the clock struck nine [this was the equivalent of 12 o'clock where his mother was] and suddenly I felt that a fear that had been with me always was gone. I am coming home.'

ROSALIND HEYWOOD

SIR,—In this issue will be found a paper by Mr Scott and myself concerning developments in the study of the Jones Boys' experiments. We have purposely kept this paper factual and omitted our personal views as far as possible.

Nevertheless, since I have witnessed very high scoring on so many occasions, I feel it worth recording that, although I am impressed by the developments recorded in our article, I still share the view expressed by Professor Mundle in his review of *The Mind Readers* (*S.P.R. Journal*, June 1959) that he is willing to chance my arm and say that I do *not* believe that the boys *were* using a whistle, despite the fact that there are several facts which, in the light of this theory, must arouse suspicion.

Although personal views are not of prime importance in experiments such as these, I do not think they are valueless, and I would be grateful if you would allow me this opportunity to make my opinion known: an opinion which, I believe, is shared by most, if not, in fact, all of those who repeatedly saw high scoring.

K. M. GOLDNEY

EXCERPTA

From a review by Arnold Toynbee of 'The Phenomenon of Man' by Pierre Teilhard de Chardin in 'The Observer' of 22 November 1959.

THIS is a great book . . . he sweeps away the barriers between the academic mandarins' specialised disciplines because he has a

mind that sees beyond the conventional dichotomies of thought: e.g. the dichotomy between 'matter' and 'mind' . . . matter and consciousness are the outward-facing and inward-facing facets of one and the same reality. . . . Teilhard is an ardent exponent of the evolutionary view . . . he does not combat the Darwinian account of evolution but his attention is concentrated on another aspect of evolution. . . . As he sees it, the main movement in the universe has been, and is, a groping towards consciousness. Here he is wrestling with the problem of newness. He is convinced that the emergence of a new thing means that, in a sense, this new thing will be there already, and indeed there from the beginning.

From an interview with Professor Carl Gustav Jung on B.B.C. Television on 22 October 1959.

. . . If it's [i.e. death] an end, and there we are not quite certain about this end. Honestly, one cannot be certain about it, because you know there are these peculiar faculties of the psyche, that it isn't entirely confined to space and time; you can have dreams or visions of the future, you can see around corners and such things. Only ignorance denies these facts; it is evident they do exist, and have existed always. These facts show that the psyche, in part at least, is not dependent upon these confinements. . . . And then what? When the psyche is not under that obligation to live in time and space alone, and obviously it does not, then to that extent the psyche is not subjected to those laws, and that means a practical continuation of life, a sort of psychical existence beyond space and time.

In reply to a question as to what was his own psychological type Jung replied . . . I am intuitive . . . my relation to reality is not particularly brilliant . . . I am often at variance with the reality of things.

Must modern man submerge his individuality in a sort of collective consciousness? . . . No. There will be a reaction against this trend, for Man cannot live a meaningless life.

Finally, does he now believe in God? . . . a difficult question. I know. I don't believe. I know.

NOTICES

PARAPSYCHOLOGY has been made a compulsory subject for the doctorate of psychology at the University of Rosario in the Argentine Republic. Dr J. Ricardo Musso of the Argentine Institute of Parapsychology, whose comprehensive book *On the*

Frontiers of Psychology was reviewed in the *Journal*, 38, March 1955, has been appointed lecturer for the course of studies. Fifth year students are expected to attend five hours per week and Dr Musso's syllabus covers the whole field of parapsychology with a wide bibliography.

Part 189 of *Proceedings* contains THE 'PALM SUNDAY' CASE: NEW LIGHT ON AN OLD LOVE STORY written by Lady Jean Balfour. Copies may be obtained from the Secretary. Price 12s. 6d. post-free (Members half-price).

ADDITIONS TO THE LIBRARY

The Enigma of Survival by Professor Hornell Hart. Published by Rider & Co., London, 1959. Price 21s.

Watcher on the Hills by Raynor C. Johnson. Hodder & Stoughton, London, 1959. Price 21s.

Extra-sensory Perception, Witchcraft, Spiritualism & Insanity by Alastair W. MacLellan. The C. W. Daniel Company Ltd., Ashingdon, Romford, Essex, 1958. Price 7s. 6d.

Lysergic Acid, Diethylamide & Mescaline in Experimental Psychiatry edited by L. Chalden. Based on a Symposium at the American Psychiatric Association 1955. Price 21s.

Conditionnement et Réactivité en Encephalo-Graphic. Electroencephalography & Clinical Neurophysiology, Supplement, No. 6. Price £5 15s. od.

Experiment in Mindfulness by Admiral Shattock. Faber & Faber. Price 12s. 6d.

Techniques of Spiritual Growth edited by Scroken. Princeton University Press. Price £2 8s. od.

Yoga by Ernest Wood. Pelican Books, 1959. Price 3s. 6d.

Madame Blavatsky by John Symonds. Odhams Press Ltd., 1959. Price 21s.

Mount Analogue by Rene Daumal. Vincent Stuart Ltd., London, 1959. Price 15s.

Facts From Figures by J. R. Moroney. Pelican Books. Price 5s. 1956.

Water Witching. U.S.A. by Evon Z. Vogt and Ray Hyman. University of Chicago, 1959. Price 37s. 6d.

Phénomènes de Mediumnité by Robert Tocquet. Collection Bilan du Mystere Noz. 9 Gaetan Bernoville, 1959. Price 9s.

A Review of Published Research on the Relationship of some Personality Variables to ESP Scoring Level by Gordon L. Mangan. P. F. Inc., 1958. Price 15s.